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ORIGINAL COMMUNICATIONS.

Experimental Researches applied to Physiology and Pathology.

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(Continued.)

VII.—ON THE CAUSES OF THE TORPIDITY OF THE TENREC, (*Erinaceus ecaudatus*, Linn.)

The influence of cold as a cause of hybernation has not been considered essential, on account of an assumed fact, which is altogether incorrect. This fact is that a mammal called the Tenrec, (*Erinaceus ecaudatus*, Lin.,) which lives in the Islands of Mauritius, of Madagascar and Bourbon, is torpid under the joined influences of warmth and dryness. Cuvier says, about the Tenrec: "They are nocturnal animals, remaining in lethargy three months each year, although they inhabit the torrid zone. Moreover, Bruguiere asserts that it is during the time of the greatest heat that they are torpid." Physiologists have admitted this fact as true, and they have supposed that warmth and dryness could be, like cold, a cause of torpidity.

The following facts prove that the torpid sleep of the tenrec takes place from precisely the same cause as that of the hedge-hog, (*Erinaceus Europeus*, Lin.,) and of other hybernating mammals:

1st. The tenrec and the hedge-hog belong to the same family, and they are much alike.

2d. According to MM. Desjardins, Telfair and Coquerel, and to my own observations, the tenrecs earth themselves, and are torpid from the month of June to the month of November, *i. e.* during the winter season of the Islands where they live.

3d. Hybernating animals, belonging to varied species, observed by Pallas, Mangili, Marshall Hall, Berthold, Barkow, and myself, have been found torpid at the temperature of 61 to 66° F., (16 to 19° Cs.) Moreover, I have found that dormice, (*Mus glis.*, Lin.) even at a temperature of 68 to 72° F., (20 to 22° Cs.) may become torpid, and I have observed some that were constantly sleeping, during a whole week, at a temperature varying from 59 to 68° F. (15 to 20° Cs.) I have stated that hedge-hogs may be torpid during the summer; in Paris, at a temperature of 68 to 72° F., (20 to 22° Cs.) and lately, in Philadelphia, I have seen a marmot (*Arctomys monax*, Buff.) torpid, in June, at a temperature of 71 to 73° F. (21.5 to 23° Cs.)

4th. During the time of its torpidity, the tenrec is under the influence of a temperature of 59 to 72 or 73° F., (15 to 22 or 23° Cs.) rarely more, and sometimes less. Therefore these animals are exposed to a temperature sufficiently low to render them torpid, for that temperature may produce that effect on the hybernating animals of cold countries.

From these facts I believe it is right to conclude that torpidity is induced in the tenrec, in the same way as in the other hybernating mammals, and therefore it is not necessary to suppose that warmth and dryness produce torpidity.

VIII.—ON THE INFLUENCE OF POISONS UPON ANIMAL HEAT AS A CAUSE OF DEATH.

Prévost and Chossat, and after them, M. Magendie, have ascertained that death occurs quickly in mammals when their temperature is notably diminished. My experiments confirm

the correctness of that statement. The diminution of animal heat in mammals, is so dangerous that, in one case, I have seen death take place in a rabbit after a diminution of only 22° F. (12° Cs.) I have never observed any animal continuing to live when I had diminished its temperature more than 44° F. (24°.5 Cs.) I have found the law established by Chossat perfectly correct, according to which the diminution of animal heat necessary for killing is less and less, in proportion to the rapidity with which that diminution takes place.

It is very probable that in all the cases where, in consequence either of disease, or of a wound, or of poison, the temperature of man is diminished many degrees, his life is in danger from the very fact of that diminution. It is thus in cholera, in sclerema, in certain cases of palsy, in cases of great disturbance of the respiration, in fractures and luxations of the vertebral column, in the cervical, and even in the dorsal regions, in cases of profuse hæmorrhage, and in many cases of poisoning when death is not rapidly produced.

It has been long known that temperature is diminished in poisoned persons; and there are but few cases of poisoning on record in which it is not said that the patient was cold. Chossat has found that a dog, into whose veins he had injected opium, had its temperature diminished from 105° to 62.6° F. (40°.3 to 17° Cs.), 22 hours after the injection. Brodie has found that many poisons act upon animal heat so as to diminish it considerably. Demarquay and Duméril, junior, and later, these two experimenters, joined with Lecointre, have found the same thing as Brodie in many toxic agents. I have made very numerous experiments on this subject, and some of their results have been published before the last papers of Demarquay, Duméril and Lecointre.*

I have stated that many poisons, either injected in the veins or absorbed by the vessels of the skin or of the digestive canal, may diminish sufficiently the temperature of Guinea pigs and rabbits, to produce death. This occurs when the dose of the poison is not large enough to kill in less than four or five hours. These poisons may kill only by their action upon

* See Gaz. Med. de Paris, 1849, t. iv., p. 644.

animal heat. It may be so with opium, cyanhydric acid, the cyanide of mercury, hyoscyamus, digitalis, belladonna, tobacco, euphorbia, camphor, alcohol, acetic, oxalic, sulphuric, azotic, chlorohydric acids much diluted, and some oxalates.

Of course the action of these poisons is the greater, the colder the atmosphere; but it is not always immediately so, and instead of diminishing the animal heat, many may increase it for a time, especially when the temperature of the atmosphere is elevated.

I have discovered that a dose of one of these poisons, sufficient to kill an animal, when there is no obstacle to the diminution of its temperature, may be unable to destroy life when the temperature of the animal is maintained by artificial means to its normal degree, or not far from it. My experiments have been conducted as follows:

Equal doses of poison were given, simultaneously, to two animals, as much alike one another as possible. One of them was left in a room at a temperature of from 46 to 50° F. (8 to 10° Cs.), and the other was kept not far from a chimney, in a place where the air was at from 75 to 86° F. (24 to 30° Cs.) The first was dead after a certain number of hours, or sometimes one or two days, having its temperature much diminished. The other, on the contrary, had no perceptible diminution of its temperature, and was generally cured very soon. Therefore, when taken in certain doses, many poisons may kill only by their influence on animal heat, and physicians, in cases of poisoning, should try as much to prevent the diminution of temperature, as to expel the poison or to act against it by an antidote, by pulmonary insufflation, or otherwise.

In experiments which I have made lately on the action of very pure digitaline, that had been prepared by M. Quévenne, the celebrated chemist, who has made such interesting and accurate researches on digitalis and the substances of which it is composed, I have found that this poison may also diminish temperature. I believe it is easy to explain the contradiction existing between Traube and Stannius, as regards the influence of digitalis on animal heat. When the atmosphere, in which the animal is, is cold, then its temperature may be diminished by digitalis or digitaline, but when it is warm the diminu-

tion does not take place, or it is very small. But, of course, if the dose is sufficient to kill very quickly, then it is indifferent whether the atmosphere is cold or not, because there may be not time enough for the diminution of the temperature of the animal.

I have to relate another fact which, I believe, ought to be considered as analogous to the preceding. It is that kind of poisoning which occurs when a layer of oil, of varnish or of gelatin, is put on the skin of a warm-blooded animal. Death, then, is very probably produced by a substance unknown until now, and which is secreted by the skin. The layer of oil, varnish, or gelatin preventing that secretion taking place, that unknown substance becomes accumulated in the blood, and then are produced the phenomena so well studied by MM. Fourcault, Becquerel, Breschet, and Magendie. I have found that in such a case the animals may live, if the atmosphere in which they are kept is at a temperature inferior to 79 or 80° F. (26 or 27° Cs.) In these circumstances their temperature is not sensibly diminished, while it diminishes much when the atmosphere is cold. Therefore it is especially by their loss of warmth that animals are killed, when their body has been entirely covered with oil, varnish, or gelatin.

IX.—ON CERTAIN ACTIONS OF COLD, WARMTH, AND LIGHT UPON THE CRYSTALLINE LENS.

Pourfour du Petit has discovered that after the death of a mammal, it is not uncommon to find the crystalline lens opaque. He has found also, that when the lens has become opaque, if we draw it near the flame of a candle or a lamp, it becomes transparent again after a few minutes. The same lens, put alternately near and far from the flame, may become alternately transparent and opaque. I have endeavored to discover whether it is the light or the warmth of the flame which renders an opaque lens transparent. I have placed between the lens and the flame a layer of mineral salt, which is athermane and transparent. Then the light of the flame could reach the lens, but not its warmth. There has been no action. In another experiment I have exposed to the light passing through the mineral salt a fresh crystalline lens, still perfectly transparent. It became opaque. Now, in a third experiment, I have compared two

crystalline lenses, perfectly transparent, one exposed to the action of light passing through the mineral salt, and the other kept in an obscure place. The former became opaque much quicker, and considerably more than the other. Therefore, 1st. It is not light which renders transparent an opaque lens; 2d. Light does not prevent a transparent lens from becoming opaque; 3d. Moreover, light appears to accelerate, if not to produce, the opacification of the crystalline lens.

As to the influence of warmth, it is certain that it is that which renders transparent an opaque lens: 1st. When left in the atmosphere, at a temperature superior to 70° F. (21° Cs.), a fresh transparent lens remains transparent. 2d. When a lens has become opaque, it is frequently sufficient to keep it exposed to the warmth of the hand, during some minutes, to render it transparent. 3d. The lower the temperature of the atmosphere the quicker transparent lenses become opaque.

These experiments give a very interesting result, *i. e.*, that light appears able to produce an effect precisely opposite to the effect produced by warmth.

From these researches I conclude:

1st. That light, and a low temperature, are favorable conditions, if not direct causes, of the opacity of the lens in warm-blooded animals after death.

2d. That a warm temperature, *i. e.*, a temperature superior to 70° F. (21° Cs.) is a cause of the change occurring in the lens, by which, when opaque, it becomes transparent.

I will add, that sometimes an analogous opacity takes place in the cornea.

X.—ON THE NORMAL DEGREE OF THE TEMPERATURE OF MAN.

The degree of animal heat, in the human species, is stated as being between 98.5° and 100° F. (37 and 38° Cs.) I intend to prove that it is higher.

It is impossible to take directly the temperature of most of the internal parts of the body in man; therefore many physiologists, in order to discover the temperature of these parts, have argued as follows: According to J. Hunter, the temperature of the rectum in animals is the same as that of the right ventricle of the heart; hence it has been concluded that as the tem-

perature of the rectum, in man—still according to Hunter—is 98°.42 F. (36°.9 Cs.) the temperature of the internal parts of the body ought to be between 98 and 99° F. (36°.67 and 37°.22 Cs.)

Some other physiologists, noting the temperature of the mouth under the tongue, and supposing that this temperature is nearly the same as that of the internal parts of the body, have concluded that the temperature of man is between 99 and 100° F. (37°.2 and 37°.8 Cs.)

We will prove that these deductions are not right:

Firstly, the degree of the temperature of the rectum is not, as Hunter says, 98°.4. It may be so in debilitated men, but in healthy persons it is more elevated. It is between 100 and 102° F. (37.7 and 38°.89 Cs.) according to my own researches and to those of Berger and Maunoir. Besides, many experiments on dogs, rabbits and guinea-pigs, have shown satisfactorily to myself that the temperature of the rectum is not equal to that of the right ventricle. This last organ is from 1 to 3° F. (0.56 to 1.7° Cs.) higher than the rectum.

The temperature of the rectum, in man, being between 100 and 102° F. (37°.7 and 38°.8 Cs.), and the temperature of the right ventricle, in man, being from 1 to 3° F. (0.56 to 1.7 Cs.) higher than that of the rectum, if we suppose the same difference existing in man as in mammals, it follows that the temperature of the right ventricle of the heart in man ought to be between 101 and 105° F. (38°.33 and 40°.56 Cs.)

But a closer approximation of the exact temperature of the internal parts of the body, in man, may be obtained by taking the temperature of an organ situated deeper than the rectum and, consequently, less exposed to the influence of the atmosphere. Such is the case in the bladder. I have observed its temperature by taking that of the urine at the moment of its emission and before any sensible change had occurred in it. The urine was directly received in a vase dipped into a large quantity of water at 98° F. (36°.7 Cs.) Thus I have ascertained that the mean temperature of the urine, in man, is 102°.6 F. (39°.2 Cs.)

Now if we take notice of this well-known fact, that the temperature of the lower part of the abdomen is less elevated than the upper part, we are authorized to believe that the temperature

of the central parts of the body, in man, is very near 103° F. (39°.5 Cs.)

My experiments on the temperature of urine were made on ten strong sailors, in the spring, on the Atlantic Ocean, between the 43d and 45th deg. of north latitude. The lowest degree of the temperature of urine which I have observed, was 100°.9 F. (38°.3 Cs.); the highest was 103°.2 F. (39°.56 Cs.) My own urine, examined more than thirty times, in the most varied conditions, has been nearly always at the same temperature; the variations have been only between 102° and 102°.8 F. (38°.89 and 39°33 Cs.) The ordinary degree is 102°.5 F. (39°.17 Cs.)

Long before my researches, the temperature of urine, in the human species, had been taken by some observers, but in general without sufficient care. The temperature of the urine is 94°.25, according to Braun; 98°.9, according to De Lisle; and 103°, according to Hales. Recently Berger has taken the temperature of urine in the bladder in five women. He has found it equal to 101°.48 F. (38°.6 Cs.)

As the temperature of woman is a little inferior to that of man, the result obtained by Berger is in accordance with mine.

If we take the average between the temperature of the urine of man as I have found it, and that of woman as Berger has found it, we have a number very near 102° F. (38°.9 Cs.)

From these facts we draw the conclusion that the temperature of the thoracic and abdominal viscera, in the human species and in both sexes, is between 102 and 103° F. (38°.89 and 39°.44 Cs.), *i. e.*, some degrees higher than it is generally admitted.

XI.—ON THE INFLUENCE EXERTED UPON THE GENERAL TEMPERATURE OF THE BODY BY A CHANGE IN THE TEMPERATURE OF ONE OF THE EXTREMITIES.

The following sentence is given, as an axiom, by Dr. W. F. Edwards: "We cannot either raise or lower the temperature of any one part of the body, without all the other parts of the frame being affected and suffering a corresponding rise or fall in temperature, more or less, according to circumstances."*

* Article *Animal Heat*, in Todd's Cyclop. of Anat. and Physiol., 1839, t. ii., p. 660.

Expressed in such terms, we accept this law as perfectly true. But the author elsewhere gives an extension to this law, which we will prove to be incorrect. No doubt when the temperature of the blood, coming to the heart from a remote part of the body, has been modified in that part, the thoracic viscera ought to have their temperature modified; but to what extent? It is on this very important point that we do not agree with Dr. Edwards. He was of opinion that the influence exerted by a small part, on all the other parts of the body, was considerable. He says that the chilling of a single part, such as the hand or the foot, may cause a loss of temperature in all the other parts of the frame, even far beyond what could have been presumed as likely or possible, and that in a number of experiments where one hand was plunged in water cooled down by ice, the other hand, which was not subjected to the action of the cold bath, lost nearly 5° R. (11°.25 F., 6°.25 Cs.) in temperature.

It will be easily understood how important it would be that practitioners should be able to act on the general temperature of a patient, so as to increase or diminish it, by means of a hand or a foot bath. I regret to say that if the facts observed by Edwards are exact, his conclusion nevertheless is incorrect.

I have performed, both alone, and with the help of Dr. Tholozan, Physician of the Hospital *Val-de-Grace* at Paris, numerous experiments, with the view of knowing whether Dr. Edwards was right or not. I have found that the chilling of one hand plunged in water at the temperature of freezing point, acted very strongly on the temperature of the other hand. But, at first, there is no regularity at all in the quantity of degrees of temperature lost by the hand which remains out of the water; and secondly, we have found once that this hand did not lose any fraction of its temperature. In one case we have observed that the hand kept in the atmosphere did lose 22° F. (12° Cs.) in seven minutes. The ordinary loss of temperature has been of between 6 to 8° F. (3.33 to 4°.44 Cs.) In one case there has been only a loss of 2° F. (1°.2 Cs.) In another case there has been no loss, and, on the contrary, there has been an increase of temperature of 1°.4 F. (0°.8 Cs.)

If, like Edwards, we consider the loss of temperature of the hand not plunged in water as a sign and as a measure of the

diminution of the general temperature of the body, we must conclude, from our experiments, that the chilling of a small part of the body may be unable to diminish the temperature of the body sensibly, and that, in other cases, it may act extraordinarily upon it. But the supposition that the hand kept in the atmosphere, was able to give a measure of the modification of the body is incorrect. By taking the temperature of the mouth during the time that one hand was dipped into very cold water, Dr. Tholozan and myself have ascertained that the temperature of the body does not sensibly change. The greatest diminution of the temperature of the mouth has been nearly 1° F. (0°.6 Cs.), and this only in one case. In that experiment in which the hand not plunged in water lost 22° F. (12° Cs.), the temperature of the mouth was not diminished more than the fifth of a Fahr. degree.

As it is quite certain that the temperature of the body is constantly changing, it is easy to understand why we do not find a small but a constant diminution in the temperature of the mouth when one hand is subjected to a notable chilling. When, under the unknown influences that are constantly modifying the temperature of the body, there is a tendency to increase that temperature, then the tendency to its diminution originating from the chilling of one hand may exist without producing any effect. We have there two causes acting in opposite directions, and if they are equal they annihilate each other. If they are unequal, we perceive only the difference between them. When these two causes act in the same direction, then their efforts are added to one another, and it is probably in a circumstance of that kind that I have once found a diminution of 1° F. (0.°56C.) taking place in the mouth.

We have now to examine how the diminution of the temperature of a hand is produced when the other hand is dipped into cold water. *A priori* it is evident that the chilling of the hand kept in the air exists in consequence, either of the arrival of a cooler blood, or in the diminution in the quantity of blood. That hand being exposed to a cold air (for it is only in such a case that the experiments succeed) loses its temperature by the action of that cold air. At first the blood that arrives in the hand is not cooler, as Edwards had supposed it was. This we prove by the

fact that the temperature is but very little changed in the mouth. The supposition then remains that the quantity of blood arriving in the hand is smaller than usual. This may happen by two modes, one of which is that the heart sends less blood, and the other that the blood-vessels of the hand are contracted and prevent, in part, the passage of blood. It is certain that the heart continues perceptibly to send the same quantity of blood. Therefore we are induced to admit that the hand's blood-vessels are contracted. But now what is the cause of that contraction? We will try to show that it is in an action of the nervous system. Every one knows that under the influence of a sensation or of emotion, the hands, and sometimes the feet, become cold. The nervous system, in consequence of that sensation or emotion, acts upon the blood-vessels and excites them to contract. The calibre of the visible vessels is sensibly diminished. The same phenomena take place in the two hands when one is dipped into very cold water. An exceedingly violent pain is felt, the nervous centres are strongly excited, and they act then as under the influence of an emotion. Dr. Tholozan and myself have observed that the greater the pain felt, the more the temperature was diminished in the hand left in the air.

As to the influence of partial heating Dr. Edwards relates the following experiment:

"The hand being immersed in water heated to the temperature of 34° R. (108°.5 F.—42°.5 Cs.) rose one degree of the same scale, and the temperature of other remote parts not immediately exposed to the influence of heat were found to have risen to a corresponding degree."

I have repeated this experiment of Dr. Edwards, and I have found no evident elevation in the temperature of remote parts, as the mouth and the hand, not immersed in water.

I conclude, then from the facts contained in this note, that, in general, the temperature of the body is not sensibly modified by the chilling or the heating of a small part of the frame.

XII.—ACTION OF COLD ON THE COAGULABILITY OF BLOOD, AND PERSISTENCE OF LIFE IN FROGS AFTER LOSING HALF OF THE VENTRICLE OF THE HEART.

Dr. Marchal (de Calvi) and other physiologists, have recently asserted that cold diminishes the quantity of fibrine and conse-

quently the coagulability of blood. I have observed a curious fact, which shows that blood may possess a very great coagulability although exposed to the action of cold. I have found that after the removal of the half of a ventricle, in frogs, the blood may coagulate at the surface of the wound and the animal may continue to live. After the mutilation has been made, the lips of the wound are drawn upwards and inside in consequence of the muscular contraction. The blood flows very abundantly, but its coagulation quickly begins, and a layer of solidified blood is soon formed on the entire surface of the section, and by this process the wound is rapidly obliterated. The hemorrhage has been frequently stopped in a few minutes.

This experiment is successful only in cold seasons, probably because the batrachia are able to bear much better a loss of blood at a low than at a high temperature. The pulsations of such mutilated hearts continue with regularity and strength, but the impulse given by the remaining of the ventricle ought to be diminished. Nevertheless the circulation of blood is accomplished well enough to allow the animal to live many months.

I have shown to the *Société de Biologie* at Paris, two frogs on which the wound of the heart was cicatrised after fifteen days.*

XIII.—ON A SINGULAR CASE OF ANIMAL GRAFT.

Every one knows the experiments by which the cock's spurs or many other animal textures have been grafted on the body of an animal, and especially on a cock's comb. I have succeeded in grafting the tail of a young cat on a cock's comb. I performed this experiment in France in 1850.

After having divided the tail of a young cat, I made a longitudinal section on a cock's comb, and I united these two parts one to the other, by stitching the cut surface of the cat's tail to the cut surface of the cock's comb. The skin of the cat's tail had been turned a little over itself, so that its internal surface was in contiguity with the cut surface of the cock's comb. Eight days after, I punctured the skin of the tail at a distance from the cock's comb, and blood escaped, so that it was evident that circulation was already established. The tail had been cold during

* See Gaz. Méd. de Paris, 1850, t. v. p. 169.

all the day of the operation, but it became warmer gradually from the second day. The union appeared much advanced on the third or fourth day. The tail was entirely fixed on the eighth day.

Unfortunately, on the eleventh day, the cock had a fight with another cock, and the cat's tail was torn out from the ground on which it had been fixed. I was thus deprived of the opportunity of knowing what transformations should have taken place in the tail.

By examining it I found that all its tissues were fresh, and that its blood-vessels contained blood.

XIV.—ON A CONVULSIVE AFFECTION PRODUCED BY CONSIDERABLE INJURIES OF THE SPINAL CORD.

I have discovered that a very violent convulsive affection is the constant result of a considerable wound of the spinal cord, in certain animals. It is more especially in guinea-pigs in whom a transversal section of a lateral half, or a complete section of the spinal cord has been made, that this is the case.* The section was made at the level of one of the last six dorsal vertebræ or the two first lumbar.

When the convulsive fit begins, the muscles of the face and neck are the first which contract. The convulsions occur alternately in all the muscles of the eye, the face, the tongue, the jaws and the neck; so that the cause of these movements ought to act on the parts of the encephalon, from whence the facial, the trigeminal, the hypoglossus, and the motor nerves of the eyes originate. The head of the animal is alternately bent on both sides of the body, and lastly the limbs are agitated in every direction. In the case of a section of the lateral half of the spinal cord, in the lumbar region or at the end of the dorsal region, three limbs only are strongly convulsed; the two anterior and one of the posterior—the one that is on the side of the body opposite to the side of the section of the spinal marrow. The other posterior limb has only very slight movements. When the spinal cord has been entirely divided in the dorsal or lumbar regions, the two anterior limbs only are convulsed, with the muscles of the face and neck. These con-

* See Gaz. Med. de Paris, 1850, t. v. pp. 651, 895.

vulsions may come without any exterior excitation; but it is very easy generally to provoke them by frightening the animal, or by pinching, burning or otherwise exciting it. The part of the body upon which a mechanical excitation acts more powerfully is the skin of the face or the neck. This convulsive affection has a great analogy with epilepsy; but it has also some distinctive features,—for instance, the animals, during the convulsions, do not appear to lose their consciousness, and they frequently cry when they are pinched.

The affection begins generally eight, ten or twelve days after the spinal cord has been wounded. The convulsions then are not very strong, and it is usually four or five weeks after the operation that the fits are violent and easily produced. Three or four months after, the violence and frequency of these convulsions are diminished; yet in two or three instances they have increased in strength and frequency more than a year after the operation. The affection may last a long time: in one case it still existed two years after the operation.

Generally the fits last from five to fifteen minutes. The stronger they are the shorter is their duration. For an hour or two, and sometimes for one or two days after a violent fit, it is impossible to produce another one by any kind of excitation. When a fit takes place a short time after a violent one, it is always weak or very short.

I have made the following experiment on some guinea-pigs having this convulsive affection: I put them in a small box, so that they had but little room to move, and I gave them food in great abundance. They then ate very much and were deprived of exercise, and, in consequence of that mode of living, they had exceedingly frequent convulsions. One of them had a fit nearly every quarter of an hour. These fits sometimes were strong, but they did not last long. The others had fits three or four times every day. I changed their mode of living, and put them all in a large room where they had only a small quantity of food. The influence of this new regimen was nearly immediate; the same day the number of fits was diminished, and in the course of the third week afterwards they had only one or two fits; and before long some of them were cured.

I must relate also a very interesting fact analogous to the

preceding. In several guinea-pigs and frogs, which had the spinal cord transversely divided, I have observed that when the animals remained very quiet during many days, instead of the regular reflex movements, they had a violent tetanic convulsion in their posterior limbs, when the skin of these limbs was pinched.

Such a tetanic movement never occurred in these animals when I excited the regular reflex movements every day.

I have no room in this *résumé* to try to explain the facts related; I will do it in a special paper. I will only add that all the readers of this note, who know the views of Dr. Marshall Hall about convulsive diseases, will remark how strongly the facts I have pointed out are confirmatory of these views.

XV.—ON THE RELATIONS EXISTING BETWEEN THE ORGANISATION OF NERVE TUBES AND THEIR VITAL PROPERTIES.

It is well known that when the nerve tubes are examined in a very fresh state, their contents, the *medulla*, or *white substance of Schwann*, appear pellucid and homogeneous, and of a fluid consistence; but a kind of coagulation soon takes place in that medulla, making it easily distinguishable from the tube itself, solid, grumous and much less transparent. Water has the power of producing very quickly that kind of coagulation of the white substance of Schwann, and the more quickly the warmer it is.

It was interesting to ascertain whether a nerve-fibre keeps or loses its vital properties when its contents have been transformed by coagulation. I have performed two series of experiments in order to solve this question; one on the motor nerves, the other on the nerves of sensibility.

1st. After having amputated the two limbs of a frog, I laid bare a long portion of the sciatic nerve in both. Half an hour afterwards these two nerves were still able to act perfectly well. Then a microscopical examination of some fibres from one of these nerves demonstrated that the coagulation of the medulla had begun, but was not yet complete. Ten minutes later the nerves were still capable of acting, and the microscope showed that the coagulation was complete. A similar experiment repeated a great many times has always furnished the same result.

2d. After having divided the two sciatic nerves, not far from the knee-joint, in a living frog, I have dipped the central part of these nerves into water. After a few minutes, some fibres taken from one of them were beginning to coagulate; yet these nerves possessed their sensibility. Ten minutes after, there was a complete coagulation of the medulla, while the sensibility was very little if at all diminished.

I draw from these facts the conclusion that nerve-fibres are capable of acting nearly as well when their contents are coagulated, as when they are still liquid.

This result appears to be important, inasmuch as it contributes to show that the white substance of Schwann has not a great part in nervous action. Three reasons concur in demonstrating that the essential part, the truly active one—that which possesses the vital properties—is not the white substance of Schwann:

1. We see that the vital properties of the nerve-fibres do not appear altered, although there is a very material change in this substance when it coagulates.

2. If such a change as that which takes place in this substance when it coagulates, existed in a part possessing the vital properties belonging to nerves, there should certainly be a movement or a sensation accompanying it, and yet there is none. This material change is certainly more considerable than the change effected by a slight mechanical or galvanic excitation, which is capable of producing a movement or a sensation.

3. The microscope has already proved that the white substance does not exist in all the nerve-fibres, and that it is wanting, for instance, in the very fine fibres.

Therefore if it be not the white substance of Schwann which is active in the nerves, and as it is not the *cylinder axis* which possesses the vital properties of the nerves, because it is wanting in many fibres, it results that it is the membranous layer, the paries of the nerve-tube, to which these vital properties belong, unless there is another substance, still unknown, and existing in the tubular canal of the nerve-fibre.

XVI.—ON THE PERSISTENCE OF LIFE IN ANIMALS DEPRIVED OF THEIR MEDULLA OBLONGATA.*

I. There is no part in the nervous system considered as more essential to life than the medulla oblongata. In the last few years many German physiologists have asserted that this nervous centre is the source of the rhythmical movements of the heart. Besides, an eminent physiologist maintains that in the medulla oblongata a small part exists which is the focus of vital power. Moreover, it is certain that the medulla oblongata has a great share in the respiratory movements. Therefore it seemed probable that the ablation of such an organ, even in cold-blooded animals, ought to be speedily followed by death. Such is not, however, the result of that operation; and in favorable conditions, the batrachia, for instance, can live more than four months after the loss of the medulla oblongata. During all that time, these animals, in appearance remain in good health, and I have observed in them the existence of all the following functions and properties:

1. The circulation of blood continues as well as in unmutilated frogs. The beatings of the heart are at first quickened generally during half an hour, an hour or an hour and a half, after the operation; they then return to their normal rhythm, and they are found as regular and vigorous in frogs deprived of their medulla oblongata, for several days or even several months, as in healthy frogs. Sometimes, particularly when the hemorrhage has been considerable, the beatings of the heart become less numerous and less energetic; then the animal dies very quickly, but if it lives, the movements of the heart resume before long their normal rhythm and strength.

2. The pulsations of the four lymphatic hearts take place as in healthy frogs.

3. Digestion seems to be carried on as well and as quickly in frogs without medulla oblongata as in healthy frogs. I have ascertained this fact by introducing pieces of earth-worms into the stomach of these animals, and by studying the changes produced in these aliments during their passage along the digestive canal. Although very slow, chymous transformation, absorption and the production of fæces took place.

* See the Bulletin de la Soc. Philomatique. Paris, 1849, p. 117.

4. The products of the gastric, intestinal, biliary and pancreatic secretions being very useful, if not essential to digestion, it is very probable that these secretions exist.

5. The urinary secretion and also the production of cutaneous and intestinal epithelium, are performed as usual.

6. The pulmonary respiration ceases, but the cutaneous respiration is continued. The absorption of poisons by the skin and by the mucous membranes exists as in healthy frogs.

7. The reflex faculty is energetic, and so much so that the frogs deprived of the medulla oblongata can raise by a reflex action, greater weights than healthy frogs. As reflex movements exist, I need not say that muscles and nerves have kept their vital properties. It is frequently found that the spinal cord, especially in the *rana temporaria*, becomes so excitable that the slightest irritation of the skin is followed by tetanic convulsions.

8. The galvanic current of muscles not only exists in frogs deprived of medulla oblongata, but appears to be stronger.

From these facts it results clearly that frogs, deprived of their medulla oblongata, are in full life. It is so much so that if they are compared to frogs possessing that nervous centre, they resist etherisation longer, and also live longer after the ablation of the heart.

II. The greatest differences exist in the duration of life, after the removal of the medulla oblongata, in animals of different species, as will be seen in the following table, where the maximum duration of life is indicated in sixty different species of animals:

Classes.	Species.	Duration of life.
Amphibia.	Salamanders . . .	More than 4 months.
	Frogs	
	Toads	4 to 5 weeks.
Reptilia.	Tortoises	9 to 10 days.
	Snakes	6 to 7 days.
	Lizards	4 to 6 days.
Fishes.	Eel	6 days.
	Pike, carp, tench, eel pout, barbel	3 days.
	Perch, gudgeon and others .	25 to 40 hours.

Classes.	Species.	Duration of life.
Birds.	Sparrowhawk (newly born) .	21 minutes.
	Magpie do. .	19 minutes.
	Sparrow do. .	17 minutes.
	Sparrow, yellowhammer, linnet, pigeon, fowl, duck, pintaw, partridge, moor-hen, turtle-dove (adult)	2¼ to 3 minutes.
Mammals.*	Dormouse (during hybernation)	29 hours.
	Hedgehog ditto	23 hours.
	Bull-dog (newly born) . .	46 minutes.
	Cat ditto . .	41 minutes.
	Rabbit ditto . .	34 minutes.
	Guinea-pig ditto . .	6 minutes.
	Dormouse and Hedgehog awaken in summer	4 minutes.
	Cat, rabbit, guinea-pig and dog (adult)	3 to 3¼ minutes.

The preceding table shows that after the removal of the medulla oblongata, in different species of animals, the duration of life may be reckoned by *months* for batrachia, by *weeks* for some reptilia, by *days* for other reptilia and for fishes, by *hours* for hybernating mammals, and by *minutes* for birds and non-hybernating mammals.

III. After the removal of the medulla oblongata, the most remarkable differences in the duration of life, in different individuals belonging to the same species, may occur in consequence of differences of temperature. The lower the temperature the longer is the duration of life. Thus, the duration of life in frogs may be reckoned by *months* when the temperature is between 32° and 46° F., (0 and 8° Cs.); by *weeks* when it is between 40° and 55° F. (5° and 13° Cs.); by *days* when it is between 50° and 65° F. (10° and 18° Cs.) by *hours* when it is between 65° and 72° F. (18° and 24° Cs.); and by *minutes* when it is between 86° and 105° F. (30° and 40° Cs.)

In the other cold-blooded vertebrata the differences in the duration of life, after the removal of the medulla oblongata, are not so great as in frogs, but the law is the same. This law exists also for warm-blooded animals, so much so that the differences existing between mammals of different ages and of differ-

* Pulmonary insufflation has been used only for the dormouse and hedgehog during hybernation.

ent species, are to be attributed, in part, to their differences of temperature.

IV. As the principal condition for a long duration of life in cold-blooded vertebrata is a cold atmosphere, and as the vital phenomena taking place in these animals are much diminished when they are exposed to a low temperature, some physiologists have supposed that the persistence of life for many weeks or more, was equivalent, as regards the sum of the vital phenomena, to a duration of some hours in summer, when these phenomena have a great activity.

In answer to this objection I will at first call the reader's attention to the fact that, in batrachia, deprived of the medulla oblongata, and exposed to the action of a low temperature, the heart beating, on an average, 35 times in a minute and life lasting four months—*i. e.* 172,800 minutes, it follows that, during that time, the heart has more than 6,000,000 pulsations. In summer, the maximum duration of life having been six hours—*i. e.* 360 minutes, and the heart beating, on an average, 45 times in a minute, it results that, during that life, the heart has only 1,600 pulsations—a number which is to the other as 1 is to 375.

This comparison shows how incorrect it is to suppose that in consequence of the diminution of the vital phenomena in cold weather, a batrachian that lives several months does not live more than another living only several hours in summer.

This opinion is also proved erroneous by the facts which I have already related, and which show that all the functions and vital properties existing in frogs deprived of their medulla oblongata, appear to be as active as in unmutilated frogs. No doubt that there is a notable difference between summer and winter as to the activity of vital phenomena in frogs, but if we suppose that these phenomena are in winter only the tenth of what they are in summer, and if we consider, consequently, the duration of life in winter as being only the tenth of what it is, we shall have, nevertheless, a duration in winter forty or fifty times as great as the duration in summer.

V. Now I have to examine why, in summer, the life of cold-blooded vertebrata, deprived of the medulla oblongata, is much shorter than in winter.

The principal cause of this difference is in the fact that the

cutaneous respiration, (it is known that the pulmonary respiration does not exist in animals deprived of the medulla oblongata,) which is sufficient as long as the temperature is very low, becomes more and more insufficient, when the temperature becomes more and more elevated. So that the same law exists for the cold-blooded vertebrata deprived of their medulla oblongata, and for those which are not mutilated. The following experiments concur to demonstrate the correctness of this view:

I have found that frogs deprived of their medulla oblongata live much longer when placed in oxygen than in atmospheric air. I have made two series of experiments—one in June, 1847, the other in July, 1850. In both series the frogs were put immediately after the operation, under a receiver full of oxygen. They lived from eight to fourteen days at a temperature at which life, in atmospheric air, is always shorter than six hours. The temperature was from 64° to 84° F. (18° to 29° Cs.) These frogs would have lived longer if the quantity of oxygen had been more considerable.

By pulmonary insufflation I have maintained life in tortoises deprived of the medulla oblongata, much longer than when insufflation is not used. The duration of life was 7, 12, 13 and 17 days in four insufflated tortoises. When death occurred in these four animals, they had been left without insufflation more than five or six hours, and very likely they would have lived longer had they been insufflated more frequently. In four non-insufflated tortoises life lasted 3, 7, 19 and 23 hours. These comparative experiments were made in summer in a temperature varying from 64° to 86° F. (18° to 30° Cs.) The insufflated tortoises lived longer in summer than non-insufflated tortoises in winter.

From all these facts it is evident that in animals deprived of the medulla oblongata, death is principally caused by insufficiency of respiration.

(To be continued.)

On Dysentery. By GEORGE S. UPSHUR, A. M., M. D., Fellow of Medical Society of Virginia; Member of the American Medial Association, &c. Norfolk, Virginia.

So much has been written upon dysentery, not only in the systematic works on Practice, but also in the various Medical Journals, that doubtless little is left to be said that has not already been said upon this subject. I do not propose, therefore, in this article, to write a treatise on this subject, nor to enter with any degree of minuteness into its causes, symptoms, or pathology, but simply to examine, as briefly as possible, the value of the many remedies that have from time to time been zealously urged upon our attention and to illustrate the whole, as far as my own experience goes, by the history of a few cases taken from every day practice.

Dysentery may be called a genuine *cosmopolite*, for it is confined to no fixed locality. Indeed, so common is the affection, that no physician, who has any business at all, fails to see it every week in the year. While it very frequently prevails as an epidemic, particularly amongst those who are subject to sudden vicissitudes of weather, and are accustomed to unwholesome food and ill-ventilated apartments, it cannot be said to be confined exclusively to any particular class or condition, but frequently attacks the high and the low together, and is found often as difficult of management in one as the other.

The most prolific *cause* of dysentery is undoubtedly a want of balance between the functions of the skin and the mucous surface of the bowels. No question in pathology is better established than that the skin is closely allied in its healthy and diseased action, to the intestinal mucous membrane, and indeed to all the mucous membranes, and that they frequently perform a vicarious part towards each other. Hence a cessation of the normal secretion from the skin is almost invariably followed by a congested state of the mucous surfaces, amongst which the mucous membrane of the intestines bears a conspicuous part. Sudden exposure then to drafts of cold air, when the patient is over-heated and sweating freely, is very apt to give an attack of the dysentery—especially if he should be much

fatigued at the time. Irritating ingesta, unripe and acid fruits, and drastic cathartics are also frequent causes of the disease. I have also seen the disease produced in a number of instances by the too free use of capsicum, either as a condiment, or exhibited, combined with quinia, in intermittent fever.

Dysentery is one of the few frank, open inflammations of an inportant organ that does not make its attack by rigors. *Sometimes* it is ushered in by decided chill, but generally it comes on gradually;—at first there is simple looseness of the bowels, the dejections being large, semi-fluid, and accompanied with only a little griping pain along the track of the colon. These symptoms, however, are soon followed by fever, thirst, and nausea, with small and very frequent discharges of pure blood, or bloody mucus, and pain and straining at stool—these last symptoms often continue for several days after the fever has subsided and the appetite has returned. The disease is then apt to run into the chronic form, from which result ulceration of the bowel and a hypertrophied condition of the mucous membrane, which is very troublesome to the patient, and difficult of management.

The *pathology* of the disease is embraced in the simple remark that it is an inflammation of the *mucous* coat of the large intestine. When the muscular tunic and peritoneum are involved, it is to be considered merely a complication, *and no part of the disease proper.*

Treatment.

Blood-letting.—There are few cases of dysentery,—I do not speak now of the epidemic disease,—that do not require the *free use* of the lancet, if the patient is seen within the first five days after the attack. The state of the pulse is here of less value than in any of the phlegmasiæ, and indeed scarcely to be consulted in reference to the propriety of taking blood, *if the tormina, tenesmus, and frequent bloody or mucous stools* are prominent symptoms. It is well known that the pulse is rarely found to be full, hard and bounding, in inflammation, however acute, of the mucous membranes—on the contrary, it is apt to be small, frequent, and very compressible, especially in inflammation of the mucous coat of the bowels, and therefore is well cal-

culated to mislead, if relied on solely, as is too often the case, to decide the question of blood-letting. The immediate and intimate connection of the bowels with the most important nervous centres—their abundant supply of blood-vessels, and the indispensable part they perform in the economy, would lead us to conclude, *a priori*, that, when inflamed, the force of the heart's action, and indeed all the vital powers would be depressed in a marked degree. And we find this to be the case in every day practice, but such depression should never deter us from the free use of the lancet, if other symptoms call for it. I would lay down the rule, then, that in all cases of dysentery in which there are frequent, small, bloody, or muco-bloody stools, accompanied by distressing tormina and tenesmus, if seen early, general bleeding *as far as the patient will bear it*, is absolutely demanded, no matter if the pulse does *seem* to contra-indicate it. We have only to reflect for a moment that we have here acute inflammation of an important organ, which it is necessary to arrest as soon as possible, and that the lancet is the great agent in the relief of all acute, frank inflammations, to be convinced of the propriety of the rule, without reference to the temporary depression of the pulse, produced by the intensity of the disease. I dwell upon this subject because I deem it of vital importance in the treatment of dysentery. I can call to mind at least half a dozen cases of death from this disease, in the commencement of my practice, which, after reflection, convinces me might possibly have been saved by copious blood-letting. The condition of the pulse alone guided me, without reference to other and more important symptoms. Doubtless many of my medical brethren have frequently committed the same blunder. In the treatment of disease, no single symptom, however important in itself, ought to be permitted to control the judgment of the physician. Yet how often does the pulse sit as judge and alone decide questions in therapeutics from which it would be considered by some the height of rashness to appeal.

After general blood-letting, the application of leeches or cups along the track of the colon, or of leeches to the anus, will be found of great service—but topical depletion should never be permitted in lieu of general depletion, where this last can be resorted to. Indeed, cups and leeches are of little value

in the first stage of acute dysentery, unless preceded by the lancet.

Mercurials.—No agents have been more over-rated in the treatment of dysentery than mercurials. The combination of calomel, opium and ipecac. is the *alpha* and *omega* of therapeutics in this disease, with a very large and respectable body of physicians; and such is the testimony adduced in its favor that I doubt not beneficial results have *followed* its use in particular cases. I am disposed, however, to attribute the benefits to the *opium and ipecac. only*, and am satisfied from careful and ample observation, that they would have accomplished much more good without the calomel. It may be true that in the dysentery of tropical climates, where torpor of the liver is usually a complication, mercury is sometimes administered with benefit, but in the ordinary dysentery of this country I am persuaded it does more harm than good. My own observation in this regard is corroborated by some of the best authorities in medicine.

Dr. Cheyne, in 3d vol. Dublin Hospital Reports, says: "Mercury could not be depended upon, and did not relieve in numerous instances when the mouth was affected, and sometimes seemed to increase the disease."

M. Twining, who saw a vast amount of the disease in the General Hospital at Calcutta, says: "I speak without hesitation on this subject, from having too often seen the fallacy of trusting to the effects of calomel for the cure of severe acute dysentery, and having tried that medicine extensively in every form of the disease."*

Dr. Mackintosh, (*Practice of Physic*, vol. I. p. 366, 1837,) says: "My own experience in this country, as well as within the tropics, enables me to confirm the statement of Dr. Cheyne and M. Twining."

Dr. C. A. Lee, in a note to the art. *Dysentery*, in Copland's Dictionary, says: "With respect to the use of mercury in this disease, we believe that it generally proves injurious."

Dr. Dunglison says that the common remark, that if the disease proves obstinate it will yield, provided we can only 'touch the

* Vide Diseases of Bengal, by W. Twining, Esq., p. 40

mouth' with a mercurial, does not accord with his experience. He has seen many cases in which the effect of mercury has been induced on the system, and, nevertheless, the disease has proceeded to a fatal termination. (*Practice of Medicine*, vol. I. p. 114.)

Dr. Batchelder, in the N. Y. Journal of Medicine for July 1851, confirms these statements by adding his own reliable testimony against the calomel practice.

But it is useless to multiply authorities upon this subject. I will add a case or two in illustration from my case-book:—

Case I. Was called in to see M. J. on 3d of June, 1849—a feeble man, æt. 30, who had recently been severely ptyalised from taking six grains of calomel for a slight diarrhœa. Found him with high fever, nausea, thirst, urgent tormina and tenesmus, pain upon pressure along the course of the colon, and dejections of blood and mucus every few minutes. *His gums were swollen and sore, and he was discharging saliva freely, accompanied by intense mercurial fetor.* I bled him to syncope, had him freely leeched, and administered ℥ii. of Hope's Nitrous Acid Mixture every three hours. He was almost entirely well in two days.

I have no question but that calomel in this case was the immediate cause of the dysentery.

Case II. Mrs. W. a robust lady, æt. 50, was seized with simple looseness of the bowels with slight griping, June 10th, 1851. Saw her on 11th,—fever intense, pulse rapid, soft, compressible, skin dry—thirst urgent—nausea and vomiting—pain upon pressure over abdomen—bowels moved every ten minutes, with such tenesmus as to force the patient to cry out—dejections pure blood. Venesection *ad deliquium*—Laudanum enema—Hope's mixture.

12*th*. No improvement—suffering excessive—experiences some relief as long as the enema is retained—still vomiting—ordered hot fomentations. ℞. Cupri Sulph. gr. iij.; Pulv. Opii gr. vi. M. Pil. 12—S. One pill every three hours, and continue enemata.

13*th*. As yesterday—probably a little less straining at stool—applied Emp. Canth. to abdomen; continue pills.

14*th*. Symptoms not quite so urgent—less nausea and thirst,

but still suffers intensely. ℞. Calomel gr. xii. Ipecac. gr. iv. Morph. Sulph. gr. ij. M. Pil. 8—S. One pill every two hours.

15*th.* More fever, and dejections more frequent—took all the pills—stomach very irritable—pulse frequent and feeble—ordered laudanum enema, to be repeated whenever the bowels are moved.

16*th. Found my patient profusely salivated,* with no abatement of the dysenteric symptoms. From this time kept her upon large doses of acetate of lead and opium with laudanum injections, and occasionally a full dose of Dover's powder at night. She was well on the 22d.

These two cases taken indiscriminately from a number that have fallen under my notice, show conclusively that the mercurial impression is not incompatible with the existence of acute dysentery, and that calomel is very far from being the Magnus Apollo in the cure of this disease, which its zealous advocates claim for it.

Saline Cathartics.—I have prescribed a combination of Epsom and Rochelle Salts, ʒij. of each, after general bloodletting, with the happiest effects in many cases of acute dysentery—and am disposed to think very favorably of the practice. I think it is particularly applicable to cases occurring in robust persons where the tenesmus is very troublesome. Frequently have I seen this symptom entirely relieved after the copious watery evacuations produced by the saline.

Acids.—The nitrous acid mixture of Dr. Hope,* which was introduced to the profession in this country through the columns of this Journal, by Dr. C. D. Meigs, in 1838, is justly considered by those who have used it freely, a remedy of great value in dysentery. Among all the good things which Dr. Meigs has done for his brethren, he never did a better than calling their attention to this remedy. In my hands it has rarely failed to accomplish every thing that was desired. It is not a specific, but if fairly tried I think will less often bring disappointment than any other single remedy in this frequently intractable affection. Great care should be taken not to administer it in too small doses, an adult should not take less than ʒii. at a time, and it should be continued at intervals of three or four hours until about eight doses have been given.

*℞. Acid Nitrosi ʒi. Tr. Opii gtt. xl.; Aquæ Camph. ʒviii. M.

Opium and acetate of Lead.—Since the publication of Dr. Batchelder's valuable article, before referred to, I have given the acetate of lead and opium a fair trial, and am prepared to speak as favorably of the combination as Dr. B. has done. My experience with it has been in those cases chiefly where Hope's mixture failed. I usually give as the first dose, one grain of opium with four of the acetate, and then half grain of the one with two grains of the other *immediately after each evacuation*, no matter if the bowels are moved every five minutes. Usually but a few doses are taken before relief is obtained.

Nux Vomica.—When the intensity of the acute stage has been somewhat lessened by proper antiphlogistic treatment, and the disease seems disposed to assume the chronic form, a combination of nux vomica with ipecac and opium will be found of great service. I usually give to an adult the following: ℞. Ext. Nucis Vomicæ gr. viii. Pulv. Ipecac gr. vi. Pulv. Opii gr. x. M. Pil. 12. S. One pill three times a day. Some cases which resisted every other mode of treatment seemed to be speedily relieved by this.

Local Remedies.—Blisters in dysentery have generally disappointed me—applied too early, they add materially to the excitement, not only because they are decidedly stimulant, but because they annoy the patient when called so frequently to stool—apart from this, they prevent the proper examination of the abdomen by pressure. They may be of service in the latter stage of the disease, but I doubt, after all, if the benefit obtained is a compensation for the inconvenience they occasion.

Injections of Laudanum are valuable auxiliaries, especially when the inflammation is seated low down in the bowel and the straining at stool is urgent. Not less than two teaspoonfuls of laudanum should be given in *as small a quantity as possible* of thin starch, and repeated if it is not retained. The nitrate of silver enemeta have also been highly extolled, but they have proved valueless in my hands in the acute stage—due probably to the fact that the first impression of the caustic is irritant, which obliges the bowel to eject it before its specific effect is produced. In the chronic stage, when the mucous membrane is less irritable, lunar caustic injections will be found to answer a good purpose, especially in the case of children. This remark applies

with two-fold force to those cases where ulceration of the bowel is suspected.

Diet.—All medication in dysentery will prove abortive if proper attention is not paid to the regimen. Among the most important points to be observed is *not to permit the patient to take cold drinks.* I have frequently seen all the symptoms aggravated after taking a small draught of ice water, or eating a few pieces of ice. Warm sage tea answers well for the ordinary drink, usually alleviates the thirst, and is not unpalatable to most persons. Warm mucilaginous drinks, such as the gum arabic, slippery elm, &c., are also of positive service—not only because they are nourishing to some extent, but also because they shield the inflamed bowel from irritating secretions.

During convalescence the blandest food should be taken, such as arrow-root, tapioca, soft boiled eggs, &c., and these in very small quantities. I am satisfied that convalescence is often retarded, and even a relapse induced, by putting into the stomach too much even of the most wholesome food. The digestive powers are especially feeble after an attack of dysentery, and one, who has not repeatedly witnessed the fact, would scarcely believe that a single teaspoonful too much, even of the most unirritating food, will sometimes re-produce the distressing symptoms of the disease. We can not be too cautious then in reference to the *quantity* as well as the *quality* of the diet.

During the months of May and June, dysentery prevailed to a considerable extent in this city and the adjoining counties—there was scarcely a sufficient number of cases, however, to make it epidemic. The character of the disease was decidedly sthenic—the only exceptions being in those persons who had but recently recovered from measles, or whose health was impaired by an attack of fever. The lancet, leeches or cups, Hope's mixture, opium and acetate of lead, saline cathartics, and Dover's powder were used with almost uniform success in the acute stage; and the nux vomica with opium and ipecac., the vegetable astringents combined with tonics, and proper regimen, were relied on in the chronic stage. Dr. A. T. Foster, an intelligent physician of Norfolk county, informed me that he had used creasote and oil of turpentine with good result in some intractable chronic cases. He mentioned one case which well illustrates the importance of

a rigid attention to the quantity of nourishment to be given. He commenced with a single teaspoonful of milk three times a day, and was unable for several days to increase the quantity—if he gave two teaspoonfuls, for instance, the dysenteric symptoms would be increased, and he could detect in the stools shreds of coagulated milk undigested. The patient at last was able to drink a pint of milk every day. He doubtless had ulceration of the mucous membrane of the colon.

Successful Removal of a Foreign Body from the Knee-joint.—Anomalous Character of the same. By J. WASHINGTON SMITH, M. D., of Croton, Del. county, N. Y.

MESSRS. EDITORS,—The following case presents so many points of interest, if not of originality, in its history and treatment, that I have thought a record of it worth preserving—and, such as it is, I now place it at your disposal.

Mr. R. M., æt. 22, a laborer on a farm, otherwise healthy, consulted me in June, 1851, in regard to a difficulty of the right knee. For some months previous something had appeared to "catch," as he termed it, in the joint while using it, causing excruciating pain, followed by inflammation, swelling, and weakness of the joint. The peculiar "catch" had only existed for a few months, but the patient says, "the joint has been a little weak and swollen at times ever since a 'wrench' it received some four or five years since, during a wrestling feat." Recently he had perceived a "loose body, something like a bean," each side of the patella, and at other points, which was freely moveable and would easily slip into the joint. At times it was readily detected, at others difficult or impossible. The knee was considerably enlarged, as from chronic thickening of the synovial membrane, and tender to the touch, in places.

Diagnosis.—After the above history, and manipulating the joint, so as to bring the foreign body within reach, I did not hesitate to pronounce it a case of *loose cartilage* within the joint.

The probable cause, nature, course, usual treatment, &c., were fully explained, and the patient dismissed, with the advice not to think of an operation for its removal, unless the use and safety

of the limb imperatively required it; and that efforts be made to "fix" or confine it *outside*, or else to prevent its escape from *within* the joint; also, to consult older and more experienced counsel.

At my request the patient called occasionally to report his condition, and enable me to watch the progress of the case. After a time he acquired such dexterity as to bring the foreign body outside of the joint almost at pleasure.

After September, 1851, he was not able to attend regularly to his occupation; and as the joint was becoming more thickened and stiffened, from the continued irritation to which it was exposed, efforts were made to confine the cartilage, (as I still supposed it,) by pushing it far away from the joint, in one of the synovial pouches by the side of the patella; but it was found, upon repeated trials, that it could not be retained *in situ* by any amount of pressure which could be borne, and allow any motion of the joint. Efforts were also made to *prevent* its escape from the joint, but with only partial success, while the cause of irritation was sure to be at work.

There now appeared but one other alternative—the chance of an operation—and which, owing to the state of the joint, it must be confessed was not very promising. The almost certain result of the case as it *was*, together with the *probable* and *possible* consequences of an operation for the removal of the offending body, were again fully and fairly stated to the patient and friends. Dr. Almiron Fitch, of Delhi, a distinguished member of the profession in this section of the State, was also consulted, and coincided fully in the opinion given.

Operation.—Early in March last I was requested to remove the foreign body by operation. Having enjoined rest, low diet, and occasional purgatives during the ensuing week, on the 10th instant I proceeded to remove it; Dr. Fitch kindly consenting to be present, and from whom I received valuable assistance. Some delay and difficulty were experienced in confining the same in the position desired, viz: upon the inner condyle, without occasioning too much motion of the joint. The integuments were then drawn tensely forward (about one inch) over the foreign body while the same was firmly fixed by the fingers of an assistant. A longitudinal incision, three-fourths of an inch in

length, was then carried through the integuments directly upon the cartilage, (?) but, as it could not readily be removed, a second incision was made, so as slightly to enlarge the first, when with a tenaculum it was removed, with some difficulty, owing to the thickened state of the joint, and the unexpected *hardness* of the supposed cartilage. In form it much resembled an almond, was eight lines in length, six broad, and four in thickness. Its substance was evidently osseous or calculous, but unfortunately (and contrary to agreement) it passed out of my possession before I had an opportunity of subjecting it to any chemical or microscopic tests. It was completely enveloped in healthy looking cartilage.

The hemorrhage was slight, and soon ceased, when the incision was nicely secured by plasters, a compress carefully adjusted so as to securely close the *valvular* opening, and over these a figure of 8 bandage; the whole being completed by a long splint to outside of limb, secured by roller, except over the knee, nearly preventing all motion. Water was applied several times a day, and was the only application. There was a slight exudation of serum, but none of synovia. The diet was light for a few days, and small occasional doses of pil. cath. comp. and pulv. jalap. comp. were given, to procure the regular evacuation of the bowels. Union was partly by first intention, and partly by what Macartney calls the *modelling* process, *i. e.*, without suppuration. The case was closely watched, and there was no inflammation at any time.

After a few days the patient sat up part of the time, but the plasters and splint were not dispensed with until the thirteenth day, when the wound appeared perfectly united. A compress and figure 8 bandage were continued, and directions given to use and flex the limb but slightly; but from that time he began and continued to go about. April 15th there was only a slight weakness, and some stiffness upon flexing to an acute angle. At the end of six weeks he was attending to his ordinary occupation, and was able to join, as he was wont to do, in "the giddy mazes of the dance."

Remarks.—The operation by subcutaneous incision, as proposed by Prof. Syme and M. Goyraud, was hardly practicable, owing to the thickened state of the integuments, though it would

in most cases greatly lessen the danger of inflammation. Wounds penetrating the cavities of joints, especially the larger, have ever been the dread of surgeons, and the common result an opprobrium to the healing art. This case is a striking illustration of the importance of *perfect rest* (of the joint) and of a *simple*, but not *too* antiphlogistic treatment; though in regard to the latter the previous habits should be our guide. Here there was evidently a *want* of action, though the diet was "light" for only a few days, but had it been more generous, I am satisfied complete union by first intention would have taken place—a most desirable result in wounds of joints.

The history of this case may, and will doubtless, satisfy some minds, that a *fragment* of bone was nearly or quite detached at the time of the injury mentioned; but to my mind it is not so conclusive. A section of one end of the foreign body revealed a hard, friable substance, more resembling calculus. But, was a calculus ever completely enveloped in healthy cartilage? If so, it is a most remarkable provision of nature to prevent injury to the joint. May not a peculiar abnormal state of the synovial membrane, from any cause, give rise to an adventitious growth or deposit in the synovia as well as in other fluids of the body? Loose cartilages and calculous concretions are not very usual, but I do not recollect any account of a foreign body in the cavity of a joint, similar to the present.

The patient consulted different surgeons, to whom he stated the symptoms and diagnosis; but it is a curious fact, that none of them, at the time, detected the foreign body, (though part were satisfied of its existence,) and at one time, believing it a case of chronic synovitis, counter irritants, and ung. iod. comp. were freely applied, to procure absorption!

Case of Conception before the Appearance of the Menses.

By Wm. T. Taylor, M. D., Philadelphia.

To the Editors of the Medical Examiner.

Gentlemen,—The general experience of the medical world has established it as a physiological fact, that conception cannot take place prior to the appearance of the menses. But there are instances of females becoming mothers, who have never

menstruated; they, however, must have conceived just at the time when the catemenia were about to be established, which after parturition and suckling probably occurred regularly.

The following case, which I have met with, seems to be an anomaly in the annals of midwifery:

During the month of June, 1851, I was requested to visit Hannah B., a mulatto, who was pregnant with an illegitimate child. Was much surprised at finding my patient herself was a mere child in appearance and manner. She was thirteen years of age on the 3d of the previous February, and, though she had never menstruated, was, when I saw her, in the eighth month of gestation. Her general condition was plethoric; her breasts well developed, and the areola quite dark.

On the 13th of August she was taken in labor, but in consequence of a prolapse of the funis umbilicalis, which could not be replaced, I delivered her of a still born child, of the usual size, and perfectly formed. The lochia continued for a few days, and she passed through the accustomed period after delivery very favorably.

It is now one year since, and her menses have not yet made their appearance, nor has their been any vicarious discharge; her health during the whole time remaining perfect.

Never having read of a case of the kind, I have sent it to you for publication, should you think it of sufficient interest.

Philadelphia, August 19*th*, 1852.

Case of Fracture of the Cranium. By H. J. Rice, M. D., of Rockville, Indiana.

James Medley, aged fifteen years, sanguine temperament, in September 1851, received an extensive fracture of the cranium from the kick of a horse. The animal struck him with the front part of the shoe along the superciliary ridge of the right eye. The rim of bone forming the superior semicircular margin of the right eye came away by the slightest traction, this rim extending from one angular process to the other. The external wound through the common integuments and muscles extended transversely above the eye for the distance of three inches. Sim-

ple inspection and the use of a probe disclosed that nearly the whole superior portion of the frontal bone was driven far down on the brain.

The lines of fracture were three. The first commencing at the external angular process of the frontal bone, extended in a straight course towards the same process of the left side, passing the median line towards the left, an inch and a half, disclosing the superior margin of the frontal sinus. The second ranged along the temporal ridge till it met the coronal suture. The third passed transversely from the terminus of the second to a point an inch and a half to the left of the median line. The fourth, which would complete an irregular quadrilateral, I presume, was an example of bent bone, or a linear fracture of the external table, for this side was not depressed beyond the common level. Here was a shocking fracture; nearly the whole frontal region was driven down on the anterior lobes of the brain. The bone was firmly located in its false position, and the brain covered by the dura mater presented some hernial protrusions along the first line of fracture. My best endeavors to bring the plate to the common level failed. An incision through the muscles and integuments was made along the second line of fracture. A large flap could now be raised, for the depressed portion was perfectly stripped of its integuments; the pericranium being peeled from every point, and torn into shreds, was removed. It was still impossible to elevate, for the bone was firmly and almost immovably fixed in its false position. The internal table had scaled beyond the line of fracture in the external, at some points for the distance of quarter of an inch. A small trephine and Heys' saw were brought to bear, some snags of bone removed; and with elevators, by using cautiously what seemed to be a dangerous degree of force, the whole depressed portion was elevated to the common level. The dura mater, so far as could be detected, was not pierced, but was peeled from the depressed portion. The hæmorrhage was inconsiderable; a branch of the temporal artery required the ligature. Coagula were removed, the flap of tissues brought to its natural position, and confined by sutures, a sufficient aperture being left at the point from which the lines of fracture started, to give exit to the fluids of the wound. This extensive fracture and depression

of bone did not produce corresponding psychological effects; there was a little mental hebetude.

September 5th. The patient was aware of all that transpired, answered questions, and knew his friends. There was partial insensibility to pain,—pulse 90 in the minute; skin moist. Charpie, saturated with tepid water, to be applied to the wound every half hour, was all the dressing required. Perfect rest and quietude enjoined, low diet, a saline purgative. R. Magnesiæ sulph. ℥i.

Sept. 6th. Passed a good night—bowels moved twice; pulse 100; skin moist; wound discharging and looks well; sutures removed, save one; union by the first intention in the line of incision; adhesive strips applied; rest and low diet.

Sept. 7th. Rested well during the night—pulse 100; no pain; some inflammation and redness of the flap; skin moist; healthy discharge from the wound. Ordered a saline cathartic.

Evening of 7th. Some pain in the head; irritability of stomach; restless; pulse 110; bowels not moved. Repeat the cathartic, magnesiæ sulph. ℥i.

Sept. 8th. Passed a restless night; face flushed; pulse 120; corded and tense; throbbing of temporal arteries; somewhat delirious; V. S. twenty-five ounces; magnesiæ sulph. ℥ii. antim. tartrat. gr. ¼—to be taken every hour till the bowels move freely; after which the tr. emetic to be continued, gr. ¼ every hour.

Sept. 9th. Patient somewhat restless during the night—bowels freely moved; pulse 100; compressible; no irritation of stomach from the antimony. Continue the tr. emetic.

Sept. 10. Patient rested well during the night; skin moist; pulse 100; mind clear; healthy discharge from wound; low diet and rest.

Sept. 11th. Improving,—all symptoms of inflammatory action have subsided; lint saturated with warm water to be re-applied, (having been exchanged for cold applications during the period of excitement.) Nothing can be more injurious than heavy dressings to wounds of the cranium; the lighter the better for the patient.

It is unnecessary to give the history of the case any farther;

from the application of simple and well established rules in the treatment of such cases, resulted a perfect cure.

In three weeks from the reception of this injury nothing but a cicatrix remained to tell the story of the accident.

Some ten months have elapsed since the injury—not a bad symptom has followed; sound as ever in body and mind, with a deformity by no means considerable.

I make this report, 1st, because it adds another recovery to the list of apparently hopeless cases.

2d. It proves that a cranial bone does not necessarily die when stripped of its dura mater and pericranial investments.

3d. As exhibiting the power of active depletion and tr. antimony in controlling the inflammatory process.

4th. As showing the vast amount of cerebral compression without corresponding manifestations over life and intellect.

Ice as a local Anæsthetic. By W. A. Berry, M. D. Washington, D. C.

Messrs. Editors,—I propose to make known to the many readers of your valuable Journal the application of a new local anæsthetic agent, which probably is not familiar to a large majority of them. This agent is applicable to but a very limited part of the frame, but its efficiency is such as to cause its use in all like cases. I refer to the local anæsthetic effect of ice in the removal of the nails of the toes or fingers. This most painful operation is disarmed of all its terrors by this simple means, and the patient witnesses it with as much composure as his operator. The agent was first made use of in the wards of M. Velpeau, during the past summer, in Paris, by one of his internes, and afterwards successfully applied by himself in a number of cases. The ice is powdered finely and mixed with a sufficient quantity of salt; next enveloped in a thin cloth, and the two phalanges of the great toe or thumb enveloped in it; the application should not be continued over five or six minutes, this time being sufficient to produce the most perfect anæsthesia. M. Velpeau proceeds with the operation in the following manner: Immediately upon removing the ice, the nail is divided in its length with a common sized bistoury from its free extremity to the root, then seizing

each half successively with a strong forceps, it is removed with a moderate *jerk*. The frequent necessity for the performance of this operation, and the great pain attending it when removed under other circumstances, is sufficient to cause its universal application by the Profession. M. Velpeau directs the application of compresses of cold water to the part during the first twenty-four hours; and the simple cerate dressing for a few days is all that is required.

It may be objected that the reaction under the application is such as to prevent its use; I will simply say that of the six patients that I saw operated upon by M. Velpeau, no such accident occurred to any one of them; and to the one case in which we applied it but a few days since, (and which has suggested this communication,) we have reason to believe that the agent is free from any unhappy results.

The simplicity and efficacy of this piece of minor surgery, and the so frequent necessity of some surgical interference in these cases, has induced me to send you this communication.

CLINICAL REPORTS.

Clinic of Jefferson Medical College. Service of PROFESSOR DUNGLISON. *February*, 1852.

(Reported by J. AITKEN MEIGS, M. D.)

We to-day direct your attention, in the first place, to a case of intermittent fever.

W. J. R., æt. 18, comes to us laboring under that particular form or type of this disease, known as the quotidian—the paroxysm occurring daily about 10 o'clock in the morning. He has been suffering from the affection about two weeks, and although recourse has been had to the great antiperiodic,—quinia,—it appears to have exerted but little of its controlling influence on his disease. One reason for this may be his continued residence in a malarious district. He lives in the south-western limits of the city,—near the Schuylkill river,—a locality in which, during the vernal

and autumnal seasons, intermittent and remittent affections are still very common; though, in consequence of the improvements lately made in this section of the city, by no means as prevalent as formerly. The banks of the river, throughout their entire length, were at one time so rife with the malarious principle,—whatever that may be,—as utterly to preclude all idea of a protracted residence in the vicinity, and many of the country mansions along the river, erected and owned by citizens of this town, were rendered, thereby, entirely untenable during a great part of the year.

The autumn generally presents us with a larger number of intermittents than the other seasons of the year; and I am inclined to consider, that the present case, though occurring, as you see, in the winter season (February), really originated in the previous autumn, and has been elicited, or made itself apparent at this late period, in consequence of accidental circumstances. Indeed, some writers are of opinion, that in all these cases, even in those occurring in the spring, the primary impression is made during the autumnal months. Exposure to the malarious cause, during the fall, may be insufficient to produce immediately an open attack of the disease; but the morbid impression after remaining in the system dormant, as it were, through the winter, is suddenly brought into active operation by the slightest excitation, as exposure to cold, heat, unusual fatigue, mental inquietude, &c., and the febrile paroxysm follows.

I have long maintained the opinion, that it would not be improper to class intermittent fever among the neuroses. It certainly displays, in its course, very marked neuropathic phenomena. The miasmatic impression falls primarily upon the nervous system, with greater or less intensity, producing the cold or congestive stage, as a result of the extreme nervous depression or sedation. The system soon begins to react; the cutaneous capillaries gradually become filled, to the immediate relief of the congested viscera; and after a few hours of alternate flushing and chilliness, the hot or febrile stage—the stage of expansion—is fully established. Finally, the paroxysm resolves itself by a copious diaphoresis, and during the ensuing apyrexia or intermission, the patient, with the exception of a certain degree of debility, may enjoy his usual health. If the disease be not now

checked, sooner or later, according to the nature of the type, a second attack occurs; and then a third; and so on until an arrestation is produced either spontaneously or by therapeutical intervention.

The intervals between the paroxysms are of the same duration; in other words, the disease is periodical in its character. Now, although our ignorance of the nature of the laws of periodicity is very profound, yet as we see these laws manifested in remittent and hectic fevers, as well as in the disease under question,—and also in certain functions of the healthy economy, as the menstrual act,—sufficient is known to render it highly probable, that this periodicity is closely connected with the functional manifestations of the nervous system. This supposition of the nervous character of intermittent fever, is further substantiated by the fact, that the paroxysm may oftentimes be arrested by the sudden production of a moral emotion. The cold stage has been completely checked, for the time being, by fright, or by the receipt of highly joyous or very painful intelligence. Mesmerism,—the power of which depends altogether upon the impressibility of the nervous system,—will sometimes produce the same desirable effect. I recollect an anecdote, illustrative of this, told me by Ex-president Madison. Mr. M. was travelling in company with Lafayette, who had been inducted into the mesmeric mysteries by the celebrated Loutherberg, a pupil of the far-famed Mesmer, when John, the servant of the former, was seized with a paroxysm of intermittent. Wishing to test the mesmeric powers of his distinguished friend, Mr. Madison recommended a trial of the art. Lafayette complied with the advice. The paroxysm was commencing, when Lafayette stared John in the face, seating himself in front of him, and holding him firmly by the hands. In a few minutes, John declared that he felt better; and by a continuance of the process, the paroxysm was entirely prevented.

Again;—our potent therapeutical agents, tonics and antiperiodics, employed in the disease, strongly bear out the opinion of its neuropathic character. They all prominently affect the nervous system, and are valuable in proportion to the degree of impression which they are calculated to make on the nerves of gustation, and on those distributed to the alimentary canal. The correctness of this view is proved by the fact, that the salts

of quinia are much more efficient in solution, than in pill,—according to one observer, one third more. Furthermore, it not unfrequently happens, that when the sulphate of quinia fails to arrest the paroxysm, opium, in the dose of gr. ij., ~~or an~~ equivalent of any of its preparations, given at the moment when the lividity and contraction of the skin begin to announce the attack, will often effectually arrest it, in consequence of the narcotic influence, which it exerts, manifestly on the nervous system.

The propriety, then, of ranking periodical fevers with neurotic affections is founded upon the threefold consideration of the modus operandi of the cause of the disease, its periodical character, and the effects of the agents employed for its removal.

In the case before us, there is, as you can readily ascertain by percussion and inspection, some enlargement of the spleen, a common, and by some thought a universal, accompaniment of this form of fever. This increased size of the spleen is attributed with much propriety to the undue accumulation of the circulatory fluid in the vascular apparatus of the organ. Indeed its tendency to become thus congested during the cold stage is one of the reasons in favor of its acting as a diverticulum to the blood under like circumstances; since, from the great distensibility of its tissue, it is much better able than any other viscus to withstand the pressure incident to the extreme engorgement to which it is, at times, subjected in the disease under consideration. In chronic cases, however, the increase in volume is mainly dependent upon hypertrophy of the substance of the organ, or upon altered nutrition, induced by repeated and long continued engorgements.

In tropical climates, as in India, the splenic affection sometimes assumes an exceedingly dangerous and formidable aspect. The whole system deteriorates, the blood becomes depraved, and hemorrhage from the gastro-enteric mucous surface, or hydropic effusions, ensue. To this distressing condition, the name of "splenic cachexia" has been given by the practitioners of India more especially. Independently of the engorgement of the spleen, the irregularity in the circulation, in the various stages, by disturbing the healthy equilibrium between the absorbents and exhalents, frequently gives rise to œdema—another pathological

point of importance, but which is not presented in the case now before you.

As regards the treatment, but little need be said: an ordinary ague is, indeed, so readily managed, that, frequently, the aid of the physician is not sought after. During the first or cold stage of the paroxysm, as the blood manifests a centripetal tendency, producing, at times, great and dangerous congestion of certain vital organs, as the brain, lungs, &c., every attempt should be made to re-establish the sanguineous equilibrium, by inviting an afflux of blood into the peripheral capillaries. Especially should our efforts be so directed, where the nervous sedation is extreme, as well as unduly protracted. The warm bath at this period is highly beneficial, in consequence of favoring the speedy supervention of the stage of expansion or reaction, by the determination it produces towards the cutaneous surface.

In the hot stage, the cooling system must be adopted throughout; whilst but little attention, excepting to the comfort of the patient, is needed in the sweating stage.

During the intermission, as you are aware, our principal efforts for the eradication of the disease are to be made, and for this purpose the salts of quinia, as the sulphate, or some of the preparations of cinchona, constitute our main reliance. Having obtained a clear mucous surface by the exhibition of an emetic or cathartic, if either or both of these agents seem to be indicated, we administer the antiperiodic in the dose of five grains about one hour before the attack. In half an hour, the dose is repeated, and, again, about the time the paroxysm is expected. I prefer this mode of administering the remedy to that adopted by many physicians, of distributing it over the entire apyrexia.

Ten grains given in solution I have found succeed in arresting most of the intermittents that have fallen under my care in the first instance, and where the paroxysms have not often recurred—one dose of five grains given an hour, and the other dose half an hour, before the paroxysm, as in the following mixture:

R. Quiniæ sulphat., gr. xv.
Acid. sulphur. dil. gtt. xxv.
Aquæ f. ʒiij. M.

Dose, one-third.

The great object to be kept in view is to make a new impression on the nervous system, so decided as to break in upon the morbid chain of associated actions, formed under the malarious influence.

The salts of quinia were, at one time, considered to be excitant, and they were always interdicted during the hot stage, notwithstanding it had been an old observation, that when powdered cinchona was given during the hot stage, it did not appear to excite the organic actions, whilst its antiperiodic virtues were not the less manifested. Careful and well directed inquiries have shown, that full doses are decidedly sedative; and hence, instead of apprehension being entertained from their administration where inflammatory action is present, they have even been found useful in the most violent inflammatory diseases—pneumonia, for example, when occurring in malarious regions. It has been a misfortune, that therapeutists have been misled in regard to some of our most valuable drugs, and this at times owing to an improper classification of them according to their presumed operation on the system. Cinchona and the salts of quinia being tonic, it has been thought that they could not fail to be excitant, and yet all admit that there is a manifest difference between healthy tone and unhealthy excitement. The valuable drug, opium, has been withheld in numerous cases, in which its use would have been precious, because it has been improperly classed amongst excitants. It is true, that in a small dose it belongs to that division of therapeutical agents, but in a large dose it is unequivocally sedative, and, hence, admirably adapted for cases in which inflammatory action may have to be combated.

It occasionally happens, perhaps from peculiar idiosyncrasy of the patient, that quinia has not its wonted influence. In such cases, much good may be anticipated from a resort to the arsenical preparations, of which the liquor potassæ arsenitis, of the U. S. Pharmacopœia, is generally chosen. In other instances, pulverized cinchona is productive of much benefit, although its active principle may have had little or no effect on the disease. In these cases the ligneous matter of the bark, in all probability, comes in aid of the alkaloid, by the excitant impression it makes on the nerves of the alimentary mucous mem-

brane. In good cinchona, we have, besides, all the febrifuge principles which may act more beneficially than the isolated quinia.

The sulphate of quinia, though judiciously administered in the present case, has, I said, been productive of little or no good. We must seek, therefore, to impress the nervous system in some other manner. With this object in view, then, the the patient will be directed to take, at about seven o'clock in the morning, an emetic consisting of pulvis ipecacuanhæ gr. xv., to be repeated in a short time if the first dose fails to act promptly and vigorously. Half an hour or an hour after the cessation of the emesis, pulvis cinchonæ is to be exhibited in the dose of grs. xx. every half hour, up to the time of the expected attack. This plan is to be repeated three mornings in succession. By such procedure, we shall be enabled to exert a three-fold action upon the nervous system—through the emetic, the alkaloid, and the woody matter of the bark.

The result of this treatment you will have an opportunity of witnessing in a few days.

[The patient did return, and reported himself cured.]

The class will have observed, that many of the cases of intermittent have been cured by the pulvis cinchonæ administered in this manner; and I have had recourse to the antiperiodic in this form, in consequence of the sulphate of quinia being so constantly prescribed, that you have but little opportunity for witnessing the action of the cinchona itself; which, in eleemosynary institutions like our own, has the advantage, that the cure performed by it is at much less cost. I have rarely had occasion to exceed the half drachm doses given at the same time, and under the same circumstances, as the five grain doses of sulphate of quinia before referred to.

THE MEDICAL EXAMINER.

PHILADELPHIA, SEPTEMBER, 1852.

OBITUARY.

We regret to announce the death of Dr. Isaac Parrish, of this city, by dysentery, after a short illness.

Few men could be less spared than Dr. Parrish; he was essentially a working man in the profession, both in the daily routine of practical duties and in everything calculated to advance and elevate it. To the pages of this journal, as well as of many of the other medical and daily presses, he was a constant and welcome contributor, and his articles were always characterised by sound judgment and practical bearing. He died in the prime of life and in the full tide of a successful practice, beloved and respected by all who knew him.

APPOINTMENT IN THE UNIVERSITY OF PENNSYLVANIA.

Dr. Robert E. Rogers, late Professor of Chemistry in the University of Virginia, has been elected to the same department in the University of Pennsylvania. Dr. Rogers is a brother of the late incumbent, and brings to the chair all the talent and experience that will enable him to fill the post acceptably and profitably.

Drs. Austin Flint and B. Palmer, late of Buffalo Medical College, have accepted chairs in the University of Louisville.

We have received the announcement of a new Medical School in Cincinnati, called the *Miami Medical College*.

MEDICAL NEWS.

Philadelphia, Aug. 10th, 1852.

Dear Sir:—The Northern Medical Association have requested me to furnish you with the accompanying resolutions for publication in the Medical Examiner.

J. Henry Smaltz, *Secretary*.

F. Gurney Smith, M. D.

At a Special Meeting of the Northern Medical Association, held Aug. 5th, 1852, the following Preamble and Resolutions were adopted:

Whereas, An All-wise Providence has, in his dispensations, seen fit to remove from earth one of the members of our Medical Association;

Resolved, That in the death of GEORGE W. PATTERSON, M. D., the members of the Northern Medical Association painfully recognize the loss of a talented and valuable brother, and feel that the community at large has been deprived of a most useful and benevolent citizen.

Resolved, That while we bow with proper submission to our great affliction, we will remember and profit by the irreproachable example of upright deportment and sentiment, always evinced by the lamented deceased.

Resolved, That while we sincerely mourn this great public and private bereavement, our grief is tempered by the full assurance that our departed friend has entered on a happy immortality.

Resolved, That Dr. Joseph R. Bryan be requested to prepare a Biographical Notice of our deceased, lamented member, to be read before the Association.

Resolved, That as an evidence of our sympathy with their sorrow, a copy of these our sentiments be transmitted to the family of our late associate.

Resolved, That on Saturday next, at 3 o'clock, P. M., the members of this Association will assemble at the usual place of meeting, in order to attend the funeral of our deceased fellow member.

Resolved, That a copy of these proceedings be furnished for publication in the Medical Examiner and News, and in one or more of the daily papers.

BENJAMIN S. JANNEY, President.

J. HENRY SMALTZ, Secretary.

DR. JOHN HASTINGS, of San Francisco, California, in a letter to one of the editors, says that he has found a very certain and easy method of introducing Iodine into the system in cases of Phthisis by means of inhalation. A small quantity of dry iodine is placed in a tumbler or cup in the chamber of the patient and allowed to escape by volatilization. He also recommends the introduction of raw or carded cotton into the vagina as a substitute for the pessary. The cotton should be rolled up firmly, so as to fill the vagina, be well anointed, and have a piece of tape secured to it in order to remove it after a few days; after which the vagina should be syringed with cold water and a fresh piece of cotton introduced, and held in situ by a T bandage.

The same gentleman reports two cases of traumatic tetanus successfully treated by the inhalation of Chloroform.

RECORD OF MEDICAL SCIENCE.

PATHOLOGY AND PRACTICE OF MEDICINE.

An Inquiry into the proximate cause of Gout and its rational Treatment. By ANTHONY WHITE, Esq., M. B., Cambridge, late President of the Royal College of Surgeons of England.—I have for some time been engaged in preparing a work on Diet, wherein I purpose, among other things, to trace out the connection between sundry constitutional disorders, and the habitual abuses of the digestive organs in childhood as well as in the adult age. I had intended to embody in that work certain theoretical and practical views, which long experience and reflection have led me to entertain on the subject of gout; but having been strongly solicited by several professional friends not to delay the publication of that portion of my notes, I have here thrown them into the shape of a separate paper.

In venturing to propound a new theory of gout, I do not conceal from myself the hazards I incur. The very announcement of my design must, I am aware, provoke against it a formidable array of prejudice, since it is natural to predict the failure of every fresh adventurer in an enterprise so often and so strenuously essayed, and always essayed in vain. On the other hand, I submit that there is a wide distinction between what is merely improbable and what is impossible, and that, however difficult be the problem I profess to solve, at least, it involves no absolute impracticability. It is safe to reject, *à priori*, the claims of one who shall pretend to have discovered the perpetual motion, or the elixir vitæ, or to have unravelled the impenetrable mysteries of ontology; but an inquiry into the natural history of any given disease belongs to quite another category; nor does there exist any reason why science should ever halt in despair at any unaccomplished point in her proper business, which is in every instance to trace back step by step those trains of phenomena to which, as we regard them in their unvaried order of sequence, we attribute the relationship of cause and effect. In some cases, this kind of research has been prosecuted almost to its last limits, whilst in others it has stopped short at an early stage, and there remained for centuries, in spite of countless efforts to discover the missing clue to the next step. But soon or late the clue will be found, and the further step achieved; for no amount of lost labor can exhaust the persevering energies of science—no lapse of time can subject her powers to bar or proscription. How often—to use the language of Sir John Herschel—how often have "we seen obscurities, which seemed impenetrable, in physical and mathematical science, suddenly dispelled, and the most barren and unpromising fields of inquiry converted, as if by inspiration, into rich and inexhaustible springs of knowledge and power, on a simple change of our point of view, or by merely bringing to bear on them some principle that it had never occurred before to try?"

I believe that, without arrogating to myself any inordinate share of

acumen, I may affirm that, through one of those happy accidents adverted to in the foregoing extract, I have been prompted to the true answer to that hitherto unsolved question—What is the proximate cause of gout?

In addition to the ordinary opportunities of a long professional life, my means of becoming intimately acquainted with this disease have been in part of a peculiar nature, such as falls in an equal degree to the lot of few medical practitioners, and such, I may boldly assert, as no man will be inclined to envy me. Corvisart's classical treatise on diseases of the heart was the work of a man who was himself afflicted with one of those organic maladies he so ably described. The symptoms of ulceration of the stomach were vividly portrayed by Béclard, from his own sad personal experience. The connection between organic disease of the brain, and certain disorders of the sensorial functions was illustrated, as it could never otherwise have been, by Dr. Wollaston's description of his own case, which he studied with the same serene sagacity and precision as characterized every other exertion of his noble intellect. I, too, however unfitted to compare in other respects with those illustrious men, have this, at least, in common with them, that I have learned from my own sufferings some facts likely, as I trust, to prove of considerable importance to medical science.

I am the offspring of parents both of whom were constantly the subjects of gout—a disease which was inherited by their four sons. Two of the latter (twins) died at the respective ages of 45 and 46, worn out by reiterated attacks of the malady. For myself, sharing largely in the family predisposition, I very early in life began to exhibit signs of latent gout, shown in the ready conversion of common nutriment into acrid acidity; and among my earliest recollections are my mother's repeated administrations to me of magnesia and alkaline preparations, to remedy the heartburn, with which I was constantly tormented. About the age of sixteen, a fixed aching pain occupied the middle flexor tendon of my right hand near the root of the finger, preventing its flexure. In the course of a week or two, the pain in the finger ceased suddenly, and was almost immediately succeeded by a severe attack of gout in the large joint of the great toe, ushered in by all the usual precursory symptoms. The subsequent visitations of the disease have extended over a period of forty years, during which it has affected every tissue of my body. Hence, I have had abundant opportunity not only to experiment upon the gout in my own person as regards dietetics and therapeutics, but also to study its natural history under the least ambiguous conditions, whenever, as not unfrequently happened, I allowed a paroxysm to run its course, and effect its own cure. It was chiefly by noticing what took place under such circumstances that I was led to entertain those views which I shall presently lay before my reader.

But first, for the sake of clearness, it will be well to define the actual state of our knowledge as to the intimate nature of gout; and this, I think, cannot be better expressed than in the following propositions wherein Dr. Holland has comprised all that is ascertained, or to be strongly presumed, on the subject:

1. "That there is some part of bodily organization disposing to gout, because it is an hereditary disease.

2. "That there is a *materies morbi*, whatever its nature, capable of accumulation in the system, of change of place within the body, and of removal from it.

3. "That though identity be not hitherto proved, there is a presumable relation between the lithic acid, or its compounds, and the matter of gout; and a connection through this with other forms of the calculous diathesis.

4. "That the accumulation of this matter of the disease may be presumed to be in the blood; and its retrocession or change of place, when occurring, to be effected through the same medium.

5. "That an attack of gout, so called, consists in, or tends to produce the removal of this matter from the circulation, either by deposition in the parts affected by the excretions, or in some other less obvious way, through the train of actions forming the paroxysm of the disorder.

6. "That there is an intimate relation between the condition of gouty habit and the functions of the kidneys and liver, both in health and disease.

7. "And that the same state of habit or predisposition which in some persons produces the outward attack of gout, does in others, and particularly in females, testify itself solely by disorder of internal parts, and especially of the digestive organs."

The opinion that hereditary predisposition to gout consists solely in a peculiar character of the ligamentous and other associated textures, is surely untenable, although it has been advocated by some authors of eminence. The disease, however prone to affect the joints chiefly, is incident likewise to all the other fibrous textures of the body without exception. The constitutional disturbance that precedes its attacks, the many functional aberrations of the assimilating, secretory, and excretory organs by which it is accompanied,—its erratic character, and the rapidity of its transitions from one part to another,—are facts tending most strongly to the conclusion that the immediate cause of the malady is not local, but general, and that the vehicle of its diffusion over the whole system can be nothing else than the circulating fluids.

Furthermore, did we suppose that hereditary transmission of gout is identified with a peculiar condition of those solids which are the most frequent seat of gouty inflammation, its active development would then have to be accounted for in one or other of the two following ways:—Either the transmitted peculiarity in question is an actual *materies morbi* deposited in the vitiated textures, or it is such a structural peculiarity of the latter as renders them especially liable to the noxious influence of a morbid principle produced in the body by other causes. Either hypothesis leads to the conclusion that gout is a blood disease. The second of the two does this directly and immediately, for it assumes the independent existence of an exciting cause, to be brought in contact with the morbidly predisposed parts through the medium of the circulation; whilst, on the other hypothesis, it is evident that the transmitted *materies morbi* must be taken up into the blood, contaminating its

mass, and producing in it effects analogous to those caused by other animal poisons imbibed from without.

But there is another class of solids, namely, those concerned in the functions of organic life, which have paramount claims to attention in every inquiry like the present. It is evident that any inherent vice in one or other of the great chylopoietic viscera, must of necessity induce a proportional depravity in the circulating fluids. Reasoning, then, *à priori*, there is nothing unwarrantable in the conjecture that the real *fons malorum* transmitted by the gouty to their offspring, is an unwholesome blood-making apparatus. Such a conjecture, I repeat, is by no means improbable, and my own observations and reflections are all in favor of its positive truth.

On the whole, then, we may safely admit that hereditary gout is a disposition to generate a certain morbid matter within the body, whether that disposition be the effect of some abnormal organic condition, promoting its formation or impeding its due excretion, or of some transmitted impurity of blood which tends, as usual in such cases, to reproduce and continue itself by vitiating the nutritive functions.

The same disposition, but created by other causes, must obviously exist in those cases in which gout occurs as an idiopathic disease. Its individual or ancestral origin is a circumstance which may influence the intensity of its development and its pertinacity in the system, but in no way affects its intrinsic nature. Whether hereditary or not, it presents the same general characteristics, and is of course attributable to the same material agent.

Setting out, then, from this cardinal principle of a *materies morbi* circulating with the blood, we have next to investigate its nature and its origin. And here we are struck, on the very threshold of the inquiry, by the close affinity between the gouty and the lithic acid diathesis—an affinity so remarkable that a very general disposition prevails among medical writers to consider lithic acid as the true gouty poison, and to impute its presence in the system to the impaired action of the kidneys.

As to this latter notion, the arguments adduced in support of it appear to me to be based on a singular misapprehension of patent facts. The discharge of lithic acid and its salts in the urine is a salutary process; and while the kidneys are actively performing such a process, it is strange indeed to charge them with creating the offensive matter they only serve to remove. It is not from the presence of lithic acid sediments in the urine of the gouty, but from their absence, that we should be warranted in ascribing to defective action of the kidneys the accumulation of that excrementitious matter in the system. If the blood was manifestly surcharged with lithic salts or their equivalents, while none such escaped in the urine, then, indeed, we should have reached the end of our inquiry in full assurance that the kidneys were the very matrices of gout. But it is not so in reality: and the most we can venture to assert is, that the renal functions, in common with others, are secondarily affected by the cause, whatever it be, of the gouty diathesis.

I think it the more necessary to insist on this point, as it is one on which so acute and lucid a reasoner as Dr. Holland appears to have fallen into error. "The kidneys," he says, "are evidently the organs of the body upon the disordered or deficient action of which depend those changes in the circulating fluids which have the closest relation to all the phenomena of gout." He would, I think, have been nearer the truth if he had said that the kidneys are, of all organs, those whose secretions afford the most faithful and the most readily discernible evidence of the changes aforesaid.

However intimate the connection between the gouty and the lithic acid diathesis, evidence is yet wanting to establish their actual identity. If the *materies morbi* we are in search of was nothing else than lithic acid, we should naturally expect to find every considerable development of that product followed by a gouty paroxysm. But this is notoriously not the case. It is no uncommon thing to find the urine constantly loaded, during a long period, with lithic acid sediments, without the occurrence of a single gouty symptom; while, on the other hand, it is known that the gouty paroxysm sometimes occurs without the excess of lithic acid in the urine. Instances of this kind, occurring in æsthenic forms of the disease, have been mentioned by Dr. Todd in the Croonian Lectures for the year 1843:—"I have remarked," he says, "a peculiarity belonging to most of the cases of this kind that I have met with, namely, that the urine does not exhibit the abundant precipitate of lithates which so often accompanies the gouty paroxysm. In some instances there was no precipitate at all; and in others it was very slight. And the specific gravity of the urine was rather below than above the ordinary standard, indicating that no excessive quantity of either urea or lithic acid was held in solution."

The gouty poison, then, is not identical with lithic acid, but is so near akin to it that the chemical and pathological characteristics of the latter may probably yet serve as indices to guide us to the discovery of the former.

"Organic chemistry," says Dr. Holland, "has taught us how readily the elements out of which all animal matter is formed are displaced from one combination and enter into others; and how very slight, frequently, are the differences, indicated by analysis, between substances eminently noxious to the system, and those indifferent or beneficial to it. We owe, further, to recent experiments the explicit proof of what simple observation had partly shown before—the remarkable effect upon the whole mass of the blood of minute quantities of certain matters brought into the circulation—leading to the inference of analogous effects from an increased proportion of one or other of the principles accumulating or being unduly retained in the body. . . . . These circumstances, now familiar to us, do certainly not identify the material cause of gout with any of the animal excretions just named [lithic acid, urea, the lithic or purpuric salts, &c.]; but they tend to concentrate our views towards them, and give a much more specific direction to future research. The assured connection of the gouty with the calculous diathesis,—the chemical nature of the concretions and deposits in the former,—and the

evidence that these deposits often become in part a substitute for the more active forms of the disease—all concur in further sanctioning the same general view. If we cannot affirm that urea, the lithic acid, or other animal compounds circulating in the blood, give cause to the phenomena of gout, under the most cautious reasoning, we are at least entitled to assume, with some confidence, that these matters secreted from the kidneys *are the equivalents to gouty matter present in the system*,—that they have certain proportion of quantity to each other,—and that upon their balance depend all the essential characters of the disease,—its modifications being determined by various causes; some of them topical, some belonging to general functions implicated in the effects of this common cause."

I particularly invite the reader's attention to the words above printed in italics. They imply that the morbid developments of lithic acid and its salts may be due to the presence of some principle altogether unlike them in sensible properties and chemical composition.

And now we may proceed to deal with the special object of this paper, which aims at determining the primary seat and the essential nature of the disease in question. To this end I shall succinctly narrate the course of induction whereby I arrived at those views which I desire to recommend to the candid examination of my professional brethren.

Having endured innumerable visitations of gout, and having had recourse to a variety of medicaments, some of which were fearfully destructive to my general health, I at last set about watching attentively the method which nature herself adopts for the cure of this disease. Thus it frequently happened, during my forty years' conflict with my hereditary malady, that I submitted to the old plan of patience and flannel, leaving the disorder to run its course and wear itself out by its own violence. On several of these occasions I was attacked with sickness and vomiting, accompanied by acrid bilious discharges from the bowels; and these evacuations were followed by immediate relief as to every local and constitutional symptom. Sometimes the result was an entire cessation of the paroxysm; at other times the alleviation was more partial; but repeated experience convinced me that the degree of relief obtained was always proportioned to the copiousness of the bilious evacuations. Pursuing this hint given me by nature, when the spontaneous diarrhœa has been too scanty I have assisted it with five grains of calomel. These in a few hours produced copious bilious discharges; the gout departed, and I was well again.

The conclusion forced upon my mind by these facts, recurring again and again during a period of so many years, is, that not to the stomach or the kidneys, or to the impaired functions of any other viscus than the liver, is the cause of gout ascribable.

In corroboration of this view I may appeal to the character of all those medicaments which at various times have been held in estimation as specifics against gout. One property is common to them all—namely, that of strongly stimulating the hepatic functions. The *eau médicinale*, which was introduced into this country about twenty years ago

from France, was a remedy of this class. It was sold in one-drachm bottles (this was the dose), and its effects were certainly very remarkable—frequently removing the most painful attacks of gout in one night. The composition of this potent nostrum long remained a secret; it was conjectured to contain white hellebore; and I recollect the physicians of the Westminster Hospital prescribing a vinous infusion of the latter, in one-drachm doses, with great success, as a substitute for the eau médicinale. The revived use of colchicum or meadow saffron, which I believe to be the essential ingredient in the eau médicinale, has put us into possession of an invaluable antidote to gout; but how does this colchicum act beneficially? Assuredly not on the stomach, which it nauseates; assuredly not on the heart or circulation, which it distresses: but it acts on the secretions of the liver; and long personal experience has taught me that until the functions of that organ are called into vigorous play, the colchicum is worse than useless.

Latterly it has been my practice to use colchicum in combination with other medicines. When I was in the habit of taking it singly, my dose was generally about sixty drops of the wine of the seeds, repeated every six hours. After three or four such doses the bowels were acted on; the evacuations had the odor of colchicum; deeply tinted scalding bile was passed, and I was well, for I needed no more.

Now, if a spontaneous evacuation of bile operates critically to the relief of the gouty paroxysm; if five grains of calomel produce relief; if just so much colchicum or other medicine produces relief as is sufficient to cause a copious discharge of bile, then is it demonstrated that the diminished or altered state of the hepatic secretion, which is always a concomitant of gout, is not to be classed among the secondary phenomena of that disease, as pathologists have hitherto invariably supposed.

Let A and B be any two phenomena whatever; and suppose that B is never found except in company with A; then will there be reason for concluding either that one of the two is the cause of the other, A of B, or B of A, or else that both are parallel effects of some third principle. But suppose it be found that, whereas B never presents itself unaccompanied by A, yet A may exist without B, and that, when both are present, the removal of the former is invariably followed by the disappearance of the latter, then it will be manifest that A is the cause of B.

The correctness of this abstract reasoning will, I presume, be admitted without question. To apply it to the subject of our present inquiry, we have only to substitute for A and B, the phrases "impaired functions of the liver," and "paroxysm of gout."

No writer that I am aware of has ever propounded, or even surmised, the doctrine that the proximate cause of gout is a functional disorder of the liver; and I cannot overcome the astonishment that possesses me when I think that it should have been reserved for me to make such a discovery. The principle, when once divulged, appears so plain and obvious, that it is wonderful it should have been overlooked so long. Such has been the feeling expressed by several of my professional brethren to whom I have communicated my views. Seldom have my conclusions

failed in such instances to receive a prompt and full assent, and to elicit from each of my hearers the exclamation, "How is it possible I never thought of that before." But the history of science is full of examples, showing how inquirers have for ages been shut out by the filmiest barriers from the acquisition of precious truths.

The derangement of the liver which always accompanies the gouty paroxysm, and manifests itself by unequivocal signs, such, for instance, as the pale color of the fæces, is too obvious to have escaped notice. Accordingly, writers on the disease have constantly adverted, more or less prominently, to this pathological fact; but they have all failed to assign to it the position it really occupies in the train of symptoms. The tendency of their speculations has generally been to consider the disorder of the liver as consequent upon that of the stomach, whereas the converse doctrine is far more consonant with observation and with physiological principles. Acidity in the stomach is an unfailing element in the gouty diathesis. Now such a condition of that organ may, undoubtedly, react on the liver, and impede or vitiate its secretions. On the other hand, we know that a very important office performed by bile is the neutralization of the free acid, which is always developed in the stomach during healthy digestion, and is, therefore, a constant ingredient in chyme; only assuming a morbid character when it is excessive or otherwise abnormal. Hence, given two coexisting facts—acidity of stomach, and deficiency or faulty composition of bile—it will be natural to surmise that the former is the effect of the latter, and nothing less than specific proof could justify our adoption of the opposite conclusion.

It is a fact of great importance to the decision of this question, that, however the administration of antacid medicines may alleviate the heartburn and the other distressing effects of acidity in the primæ viæ, such remedies never rise above the rank of palliatives in the treatment of gout. They have not the least efficacy in restoring the healthy action of the liver; whilst, on the other hand, whatever accomplishes that object never fails to remove every other dyspeptic symptom likewise.

The liver, then, is the *officina* in which is elaborated the *materies morbi*, on which the whole train of gouty symptoms are dependent. What may be the precise nature of that poison I do not pretend to determine. That remains an interesting subject for future inquiry, to which I may venture to hope that I have given a fresh impulse and an increased prospect of success, by defining its proper point of departure, and the direction it should take. The one new leading fact which I affirm as demonstrated, is sufficient to indicate very distinctly the mode of treatment which offers the only rational hope of removing the gouty diathesis, and also to explain the success which has partially attended the various empirical methods which have been adopted for the cure of the disease.

The main object to be pursued towards the effectual cure of the gouty paroxysm, by the removal of its immediate cause, is the restoration of the natural functions of the liver, as indicated by a copious discharge of bile through the bowels. This object may be attained, more or less promptly and sufficiently, by the administration, either of calomel or

colchicum, or of some other potent deobstruent of the hepatic system. But here, as in other instances familiar to the minds of my readers, the principle of combining analogous remedies will be found strikingly advantageous. My own practice has long been to rely exclusively for the cure of gout on the following prescription, the effects of which should be carefully watched by the medical attendant:—

℞. Hydr. Chlorid.
Ext. Colchici Acet.
Ext. Aloes purificati.
Pulv. Ipecac. aa. gr. j.
M. et fiat pilula quartis horis sumenda.

Two or three of such pills are generally enough to produce a considerable disgorgement of the liver, which I then assist with one or two doses of the compound decoction of aloes. By this time the gouty paroxysm has either ceased, or there is a marked subsidence of all its distressing symptoms. The pills may then be administered at longer intervals, varying from eight to twenty-four hours, according to circumstances.

The treatment I have above described possesses the cardinal and paramount requisite of being effectual to the end proposed. In addition to this, it is important to know that the combination of calomel and aloes with colchicum, while quickening and corroborating the specific action of the latter on the liver, seems also to neutralize all the noxious properties of that hitherto formidable medicine.

In conclusion, I repeat, that what is called a fit of gout, is only a peculiar manifestation of a functional disorder of the liver; and that whatever brings about a free evacuation of bile puts an end to the gouty paroxysm.

Having assumed, in the above tract, that the cause of gout is the effect of poison generated *eventually* in the liver by an imperfect assimilation of food, or indigestion, I cannot refrain borrowing from my highly learned and intellectual friend Dr. Watson, an observation which I take the liberty of quoting from his "Lectures on the Practice of Physic," vol. ii., p. 641 :—"I need not remind you of the various ways in which extraneous matters find entrance into the blood. Poisons, under their proper shape and name; medicines, which, misapplied, become poisons; our natural food and drink, which the folly of man converts into poison; the products or dregs of the secondary assimilative process; these are common sources of impurities, more or less hurtful, which mix and circulate with the vital fluid."

Three Cases of Paracentesis thoracis in Pleurisy; two of which were successful. By Dr. JOSEPH BEYRAN.—Dr. Beyran is in favor of puncturing the thorax at a comparatively early period in pleurisy. He sometimes employs a trocar, sometimes a bistoury; the latter plan he prefers when the subject is fat or muscular. The three cases to be

related occurred in Constantinople; two of them in a hospital there, to which Dr. Beyran is attached.

Case 1. Margos, aged 28, a sawyer, exposed to alternations of heat and cold, was seized with pleurisy on February 3d, 1851. On the 9th he was admitted into the hospital, with signs of effusion in the right pleural sac. On the 11th, the respiration becoming more and more impeded, Dr. Beyran punctured the thorax between the seventh and and eighth ribs, by means of the trocar of M. Reybard. Six and a half pints of transparent serous fluid escaped. A piece of diachylon plaster and a bandage were applied. After the operation he gradually recovered, and was discharged cured on February 26th; and has since remained well.

Case 2. Serpoulic, aged 32, a washerwoman, had been ailing for some days, with a pain in the left side of the chest, when she accidentally burnt her legs on April 13th, and was brought to the hospital. The next day, the most urgent symptoms were those of extensive effusion into the left pleura. Blisters and diuretics were used, without effect. On the 16th, as the patient was threatened with suffocation, Dr. Beyran punctured the thorax. The patient being fat, an incision was made first down to the ribs, until fluctuation was distinctly felt with the finger; the bistoury was then introduced, and five pints of yellow transparent fluid were removed. The flow was then suddenly arrested to prevent the access of air; plasters, graduated compresses and a bandage were applied. In half an hour the stroke-sound was normal, except at the lower part; and the respiratory murmur could be heard. The patient was calm. The next day she was much better; and on August 18th she left the hospital completely recovered.

Case 3. Dr. Beyran was called in consultation in the case of Mlle. M. Narisen, who was seized on December 2d, with pleurisy of the right side. He saw her on the 12th, when he diagnosed effusion into the right pleural sac. The patient also had tenderness in the hypochondria and epigastrium, with nausea and vomiting. Dr. Beyran recommended paracentesis; but could not prevail on the other medical attendants to have it performed until several days after. It was then performed, when suffocation was imminent, by puncture with a bistoury. The patient appeared relieved for a time, but died exhausted in eight hours after the operation. Before her death, she complained of much pain in the region of the stomach.—*Lond. Journ. Med.*

SURGERY.

On the Diagnosis of Chronic Ovarian Tumors. By E. J. Tilt, M. D., Senior Physician to the Farringdon General Dispensary and Lying-in Charity, and to the Paddington Free Dispensary for Diseases of Women and Children.—At different times we have published, in the *Lancet* and in the *London Medical Gazette*, a series of papers on the "Pathological Anatomy of Chronic Ovarian Growths, and on their

Cause, Progress, and Termination." We now propose treating of the diagnosis of these tumors, and feel no little hesitation in beginning a chapter which must be the history of the mistakes daily committed, even by the most enlightened.

The difficulties of the diagnosis of chronic ovarian tumors arise from the form and nature of the cavity in which the abdominal viscera are contained. The more easily defined limits of other organs render the diagnosis of these diseases comparatively easy; but it was necessary, for the health of the body, and to economise the space allotted to the abdominal viscera, that they should have no fixed limits, in order that they might, in their periodical ebb and flow, appropriate each according to its peculiar wants more or less of the same cavity. This community of habitation entails a certain obscurity of position, and gives rise to a corresponding obscurity of diagnosis of the diseases of the abdominal organs. With other organs we find that any obscurity of diagnosis which might have existed in the origin, diminishes as the disease progresses, but the certainty of the diagnosis of ovarian tumors does not increase in proportion to their dimensions, because each of the neighboring viscera—womb, liver, spleen, kidney, and bladder, are susceptible of attaining to a similar bulk, and these organs must, therefore, be believed to be in a healthy state by the medical attendant, before he can diagnose an ovarian tumor.

Another difficulty of abdominal diagnosis arises from the fact, that the abdominal cavity, more than any other, harbors not unfrequently tumors of stupendous size, which have no other origin than some minute sub-peritoneal cell. In such cases we need not be surprised that a tumor, which belongs to no organ, should be, by different medical men, attributed in turn to a swelling of each of the abdominal organs in the vicinity of which it is developed.

It may be thought that we exaggerate the uncertainty of abdominal diagnosis, but we do not hesitate to say that it is more difficult to diagnose many complaints of the organs contained in this cavity than those of the chest or the head. Medical men have sometimes brought us patients said to be affected with ovarian dropsy, but who were in reality suffering from fibrous tumors of the womb, chronic peritonitis, or even *embonpoint*, particularly when the patients have arrived at the age of cessation of menstruation. All those who are engaged in active practice must be able to recall similar proofs of the great imperfection of abdominal diagnosis, and the operations of ovariotomy furnish us with wholesale examples of the errors of diagnosis made by the most eminent in our profession; errors made by men who have specially studied ovarian disease, and the individual cases of that disease which they attempted to cure radically by an operation, on the value of which they staked their medical reputation. Dr. Dolhoff, at Magdebourg, had studied his patient's case for a year; many eminent men who had seen the patient with him were convinced of the existence of a tumor, and still none was found when the abdomen was opened. The best opinions of Edinburgh confirmed Lizars in his opinion, that a patient of his was affected with an ovarian tumor. An operation was performed, but no

tumor was found. We learn from Dr. Atlee's valuable table, that out of 179 cases of attempted ovariotomy, the operation could not be completed in 34. This means in plain English, that in 34 out of the 179 the surgeons made an unfortunate error of diagnosis. In 17 of these cases other important diseases existed, which would have contraindicated ovariotomy, but as these were not detected by the surgeon, the 17 operations were performed, and occasioned the death of 14 of the patients. Adhesions of the tumors to neighboring organs are also considerable complications when they are extensive, as was the case in 62 operations out of the above mentioned 179. In other words, these 62 cases were also errors in diagnosis; for if the surgeon had suspected the existence of these adhesions, he would, in most of the cases, have declined operating, well knowing that this complication increased the difficulties of the operation, increased the ratio of its mortality, and sometimes rendered it impossible. In six cases, says Dr. Atlee, (but two others are recorded,) no tumor was found, so that in these patients the abdomen was opened, it was examined for a tumor, and none was found, and then the abdomen was sewn up. The 179 operations of ovariotomy thus afford us proof of 121 errors of diagnosis.

While, however, admitting the imperfection of our diagnosis of abdominal diseases, we must not omit mentioning that some of our means of diagnosis have lately been improved, and that others have recently been added, which, if judiciously used, permit us to anticipate a diminution of what has been well termed "ovarian perplexities."

In Professor Piorry's hands Avenbrugger's discovery has been proved to be equally serviceable in detecting diseases of the abdomen as those of the thorax. Kergaredec applied his uncle's stethoscope to the abdomen, and taught us to distinguish a sound, by which, if distinctly heard, we can be confident of pregnancy. And later still, the head of the obstetric department of our profession in Scotland, Dr. Simpson, has given us a very important instrument of diagnosis—the uterine sound, by means of which we are generally able to eliminate the uterine element from very complicated problems of abdominal morbid structure. This instrument has indeed been inveighed against, with considerable force, by some who have been peculiarly struck by cases wherein the instrument was injudiciously employed, but as a help to diagnose abdominal disease, it is undoubtedly of great utility, a conviction expressed with the greater pleasure, because we differ in some other points of practice with its illustrious inventor.

Having shown the difficulties of abdominal diagnosis, having pointed out the reason of these difficulties, and glanced at some of the means by which they may be diminished, we shall now proceed systematically in our attempt to unravel the obscurity which we deplore. Our idea of diagnosis is to distinguish some peculiar disease from all those with which it may be confounded, we shall then proceed to pass in review, one after the other, all the diseased organs with which ovarian tumors have been confounded, or, in other words, all the abdominal viscera.

Should the reader find that we are tedious in the inquiry, let him remember that in learning to detect ovarian tumors he unavoidably learns

to discover the other tumors or diseases of the structure which so frequently occur in the abdomen, and that he will thus acquire a knowledge of abdominal disease, the obscurest and the most important part of medical diagnosis, for the viscera of nutrition are those principally affected in all diseases. Let him remember that the rarity of any peculiar disease should not be an unconquerable obstacle to the sagacity of the practitioner, who knows how to *seek* for the signs of disease and how to interpret them. Acquainted with the anatomy of the visceral organs, and conversant with their functions, the medical attendant should be ready to interpret those rare cases which, in the course of practice, may perhaps "turn up" to-morrow.

Chronic ovarian tumors have been confounded with ovaritis, on account of the extraordinary size of the acutely-inflamed ovary, but the rapidity of the development of the tumor, and the fever by which it is attended, will not permit the practitioner to be easily deceived; and the rapidity of the march of the disease will soon show the disease to be acute. On account of the acute march of an ovarian cyst, it may, however, be confounded with ovaritis. In the *London Medical Gazette* we have related an interesting example of this; the fever was such as attends acute disease, the iliac pain was intense, the swelling rapid, and we should have remained convinced that the tumor was of a purulent nature, if an exploratory puncture had not been made, and a transparent albuminous fluid withdrawn. A careful examination of the patient could alone prove that, in such cases, the ovary is affected, the knowledge of the nature of the tumor being left to be determined by the subsequent history of the case, a delay little to be regretted, as in both cases the indications would be the same, that is to say, to arrest the progress of inflammation by bloodletting and mercury. Ovarian tumors have been confounded with pelvic tumors, sanguineous or purulent, developed in the recto-vaginal pouch. By means of the uterine sound it is easy to eliminate the uterus from the morbid problem, but it is sometimes difficult to distinguish an incipient ovarian from a pelvic tumor, in cases similar to those described by Dr. Seymour, where the falling of the ovarian tumor into the recto-vaginal pouch gives rise to excessive pain. A few days will generally suffice to clear up the difficulties of diagnosis, for while the distressing symptoms attendant on the position of the tumor diminish, or at least do not increase, the constitutional symptoms will increase if the tumor be purulent, and it will most likely point towards the vagina, the rectum, or the skin, in the inguinal region. In very rare cases, however, it may happen that a purulent tumor attains a large size in the recto-vaginal pouch, without the difficulty of diagnosis being diminished, as when they imitate those ovarian tumors which are quite round, central, painful, and bulge into the vagina. In a very interesting case, recorded at great length, by Dr. Camus, (*Revue Medicale*, 1838,) the patient would certainly have been considered to be suffering from ovarian dropsy if it had not been known that the disease originated in the culpable introduction of an instrument into the womb to procure abortion. The tumor took several months to attain the umbilicus. In this case an obscurity in the diagnosis was not much to be regretted as

it did not increase the difficulty of the treatment. The indications were similar in the event of either disease, and a vaginal puncture of the bulging portion which did cure the abscess, might also have radically cured the ovarian cyst, as has happened in many an instance.

Sanguineous pelvic tumors are more likely to be confounded with incipient ovarian tumors, and as they have not been described by classic authors and have been but lately investigated by French pathologists, it may be well to glance at their history. In the midst of menstrual disorders, consisting in the flow being more or less abundant and painful than usual, intense hypogastric pain arises with fever, which lasts a few days, then ensues a dull heavy bearing down pain in the hypogastric region, and a large collection of blood forms in the recto-vaginal pouch, which, by pressing the rectum in and against the concavity of the sacrum, gives rise to obstinate constipation, whilst by pressing the urethra against the pubes, the same tumor causes a difficulty or impossibility of passing water. At first fluctuation may be easily detected in this tumor, but when the blood coagulates it becomes hard, and in one instance was mistaken by Malgaigne for a fibrous tumor of the womb. The tumor often disappears by resolution, or else it may open and discharge its contents in the vagina or the rectum.

These details are sufficient to show that when ovarian cysts are so confined by false membranes, as to be developed in the recto-vaginal pouch, it may be difficult to distinguish them from sanguineous pelvic tumors, but the more rapid growth of the latter and their early fluctuation, enable the practitioner to form a correct opinion, and time will give certainty to the diagnosis.—*Prov. Med. and Surg. Journ.*

Entropium Proved to Depend on Muscular Action. By HAYNES WALTON, ESQ., F. R. C. S., Surgeon to the Central London Ophthalmic Hospital, and to St. Mary's Hospital, Paddington.—I have founded the treatment on what appears to me to be the pathological interpretation of the affection, and of which the indications are, to overcome the means of inversion by dissecting away the thick marginal portion of the orbicularis, supposing that part of the muscle to be entirely, or nearly all that is at fault, and also to remove as much of the skin of the lid as may be required by its loss to produce such tension as shall overcome and restore to a natural state, whatever unnatural position the other tissues or component parts of the lid may have acquired, from the irregular position into which they have been thrown by the muscle, and which has been made more or less permanent by such changes as inflammation would produce.

Now, as to the manner of operating: let us suppose that the right eye is to be done. An assistant stands behind the patient, and having made the lid tense by drawing it outwards and raising the brow, two incisions are made through the skin and muscle, one along the edge of the tarsus close to the cilia, and the second about the quarter of an inch above, and meeting the other at the extremities. The flap thus isolated should be dissected vertically from the one side to the other, and not

taken away by horizontal strokes of the knife, or else the muscular portion will not be effectually removed. The wound should be very carefully sponged during the operation. Any arterial jet must be checked by temporary pressure with the finger. I have never found a ligature to be necessary. The exposed surface must be inspected, and, if any muscular fibres have escaped, the forceps and knife must be re-applied. The assistant should not desist until the knife has been laid aside, for the proper retraction of the skin is essential to steady and effectual dissection. Three or four sutures should be used. The cilia might appear to be in danger of being dissected off, but in reality they are not. A part only of the dissection is over them, and by the loose cellular connexion, the muscle is readily raised from the dense fibro-cellular tissue in which they lie.

The previous observations almost demand a cursory view of the usual methods of operating, and their results; indeed, I think this paper very imperfect without it; yet I fear that its introduction would make my communication too long. I must be satisfied with making the assertion, that there is not any known method of operating for entropium, that can be depended on, except that of cutting off the greater portion of the lid, and which is as bad in principle,—nay, even worse, if the physiological relations of the lid be taken into account,—as to amputate a thigh to cure a popliteal aneurism.

Heretofore, when an operation for entropium has been undertaken with the design of removing any part of the orbicularis palpebrarum muscle, it has been executed on very different principles, and in a very different manner to that I now advocate, and with very different results. The marginal part of the muscle, the musculus ciliaris, has been untouched, and only a bit of the centre of that on the lid has been snipped out. The only exception I know of is in the practice of Mr. Key. I am astonished that the notice of the operation he performed, and which is in the *Lancet* for 1825, page 235, should have escaped the attention of our writers on ophthalmic surgery. It is very lately that I have been acquainted with it, long after I had matured my own views. I cannot say whether Mr. Key continued to execute it, and what were his ideas on the subject in later years. The report runs thus: Mr. Key considered the inverted state of the tarsus to arise from the action of the orbicularis palpebrarum muscle. With that view he determined on laying bare the substance of the lower tarsus, and dissecting off the fibres of the orbicularis. The operation was performed by first turning out the lid, and then making an incision through the skin along the whole length of the lower eyelid, at a few lines distance and below the ciliary ridge; the integuments were carefully elevated by means of dissecting forceps, and the fibres of the orbicularis thus exposed were as carefully removed; there was considerable bleeding from the parts; the portion of skin which had been raised was laid down, and the wound dressed by means of adhesive straps, with a compress applied on the skin. The result is said to be successful. Mr. Tyrrell and Mr. Travers had operated previously, the first by cauterisation, the second by removing a portion of skin without success.

M. Desmarres has not overlooked the influence of the orbicularis in producing the inversion, and he gives a case that he thinks solely due to it; but he does not recognize its general operation, and he significantly indicates how little he appreciates the principle of Mr. Key's execution, and the triumphant issue of the case, by asking, in a commentary on the proceeding, "Is it to the incision of the skin, or the excision of the muscle, that the success is owing?" Spasmodic action of the muscle is spoken of by foreign authors, but with much difference of opinion concerning its frequency in producing entropium. The usual Continental mode of operating on the orbicularis is subcutaneous incision, which seems to have been unsuccessful; that of M. Janson de Lyon is a terrible affair, the cutting out of several vertical portions of skin, including some fibres of the orbicularis. Dr. Mackenzie, in his only notice of operating on the muscle, informs us, that it may be proper, after removing the cutaneous fold, to snip off a few fibres, so as to form a firmer cicatrix, actually fixed to the cartilage; and Mr. Lawrence, a much later author, echoes the same thing, for he writes, "In an incipient case, it may be sufficient to excise a portion of skin; this remedy will, at least, answer the purpose for a time. To make the operation more effectual, a portion of the orbicularis should be removed also, that a firmer cicatrix may be produced; or the acid may be employed, using it more freely, so that its action may extend deeper, and a solid scar be the result."

A narration of cases, with the various symptoms and terminations of entropium, must be excluded from this paper, which is intended to embrace only general principles.

As trichiasis commonly exists with entropium, and the operation I have advocated bears on the united affections, I shall say a few words relative to it.

With inversion of the upper lid, there is not unfrequently decided trichiasis, and, whether the two are due to the same exciting cause, or the trichiasis have been the original affection, or merely an effect of the inversion, it matters nothing as far as the treatment is concerned; yet it may not be amiss to remark, that, with severe entropium, the edge of the lid can rarely for any length of time escape from being affected by the general inflammation, and that the cilia will the more readily retain any unnatural position in which they may be placed. And I am inclined to regard the trichiasis more frequently as an effect, because it is generally of one form, that of a separation or twisting in of the innermost of the cilia from their fellows, and that without any alteration or degeneration in the individual hairs, and because the removal of the entropium is generally sufficient to clear the globe of them. Although, practically speaking, such a degree of trichiasis matters little, it is important, before operating for the entropium, to ascertain whether there is also trichiasis, and whether the restoration of the lid to its natural position will or will not counteract the mal-direction of the cilia. Should it not be sufficient, then more skin must be taken from the lid than would otherwise have been necessary, and a slight degree of eversion of the centre of its edge produced, and which must necessarily have its limit.

When it is apparent that such moderate eversion will not suffice, the treatment must depend on the degree of the trichiasis; if it be general, the entropium and it must be attacked by one operation. The cilia must be excised at the same time, that a sufficient extent of the skin of the muscle should be removed. But when the trichiasis is partial—and, for the most part, it is—the muscle should first be dissected away, and then the irregular cilia removed. When there is doubt about the necessity of removing them, attend to the entropium alone, and observe the result, because it is not always possible before the operation for the entropium has been done, and the lid recovered from it, as well as from any inflammation and swelling that the entropium may have induced, to ascertain with exactness to what extent the trichiasis may be benefitted. If, as is commonly the case, a patient applies to be treated for entropium, with many of the cilia broken, and some just about to be restored after having been plucked out, it cannot be known what direction they may assume when growing out; and such cases should be watched.

It can be very seldom that the removal of entropium from the lower lid does not at the same time separate any irregular cilia from contact with the globe, for a single exception only has occurred to me.—*Dublin Medical Press.*

Ligation of the Primitive Iliac Artery.—In the *New Orleans Medical and Surgical Journal* for May, is reported a case of an Irishman, aged twenty-five years, who was admitted into the Charity Hospital for aneurism of the left femoral and external iliac arteries. On the 27th of March, Dr. A. J. Wedderburn ligated the primitive iliac. During the operation "severe hæmorrhage took place;" and it was found that the aneurism extended to within one inch and a half of the bifurcation of the primitive iliac artery. No chloroform was administered, and the patient complained much during the operation. On the 29th, evidence of gangrene below the knee existed. On afternoon, the 31st, the patient died—gangrene having extended as far as the hip. Post-mortem examination revealed thickening and degeneration of the tissues about the seat of the ligature, together with enlargement of the lymphatics. Dr. Wedderburn is disposed to question the success of this operation in any case; and he doubts if a cure has ever followed this questionable operation.—*New York Journal of Medicine.*

Mr. Barnard Holt's Stricture Dilator.—Stricture, in all its forms, constitutes one of the most interesting varieties of lesion which the surgeon is called upon to treat; and although its origin and the minute anatomy of its early stages, have not yet, in some points, been fully explained and examined, yet a thorough and comprehensive knowledge of the treatment of it forms an indispensable branch of study for the practical surgeon.

Being confined to no one locality or class of patients, as well as being one of the most common forms of disease, it may call for aid in districts where little opportunity for studying surgery is afforded; while it may, and often does, severely tax the patience and skill of the great hospital surgeon.

As it is generally neglected by the patient till the last moment, and as no one of the various modes of treatment recommended seems available in all cases, the most energetic attempts have been made to arrive at sound conclusions, as well as to improve upon the instruments generally employed to dilate the contracted part of the canal. Among the latter, we may fairly notice those introduced to the Profession by Mr. T. Wakley.

Notwithstanding the advances made, great disunion of opinion prevails among the Profession as to the treatment of stricture, as was well illustrated at a meeting of the Medical and Chirurgical Society, held on the 13th of April of this year. A paper was read by Mr. Cock, containing the results of the treatment of stricture by puncture of the rectum. An elaborate and highly interesting discussion followed, in which nearly every speaker dissented from the views of those who had preceded him. Such discussions as these are of the greatest service to the Profession, and show how just a view the founders of them took respecting the steps then necessary for the advancement of science. They tell us where advances are made, and converts gained,—where opinions are still divided, and how objections might be raised or met; thus giving us, in one clear and condensed view, a summary of the opinions of some of the greatest surgeons and physicians of the day; opening up fresh sources of knowledge, and bringing to light facts and opinions which otherwise might die with their possessors. Hence the records of these societies and their debates may be consulted as storehouses of information, the value of which it is impossible to overrate, and the place of which no books or lectures can ever supply.

To enter into a full discussion of the various modes of treatment adopted in stricture would be here impracticable, and, if practicable, would be quite out of place. What occupied practised debaters some hours, could not be properly examined in the columns of a journal, nor appropriately appear in an article the functions of which are strictly limited to recording the discoveries of our hospital surgeons and physicians. We shall, therefore, proceed at once to the consideration of the subject in hand,—Mr. Holt's new instrument for the treatment of stricture, or, as the inventor has named it, the stricture dilator.

This extremely ingenious instrument is, Mr. Hall says, a modification of that contrived by Perreve. It consists of a tube like a catheter, split up to the extremity, where the two halves are welded together. Running through its whole length is a directing-rod; over this directing-rod can be passed a series of tubes of increasing calibre; thus affecting the slightest to the most extensive degree of dilatation. When the small closed tube is passed through the stricture, the sounds can be passed in succession, till the highest point is attained to which the surgeon wishes to go; and, as they are passed over the guiding-rod, and down the interior of the first instrument passed, it follows that all this dilatation may be attained without disturbance to the stricture. Here, then, may be said to be in some measure combined all the advantages which other instruments have ever offered. Steady, uniform dilatation, while the urethra is not irritated by renewed friction; the absence of any neces-

sity for attempting to find again the difficult route; a continuance of the curative application, and thorough safety. The "stricture dilator" may therefore be fairly looked upon as a valuable addition to the means of treatment we possess. The following case will tend to show how satisfactorily it acts:—

J. G., a carpenter, aged 41, living in Lambeth, was admitted Jan., 1851, into Westminster Hospital. He gave the following account of his case. He had laboured under stricture twelve years. He knew of no cause for it, as the last gonorrhœa he ever remembers having suffered from had occurred eight years previously; but those who know how insidiously stricture springs up, and how obscure its origin is, will know how to appreciate these facts. However, having long suffered from it, he went at last to a surgeon, who attempted to introduce an instrument, but could not pass it more than two inches down. Considering it a very bad case, he recommended the patient to go into the hospital.

When admitted, the stream of urine was very narrow, not being thicker than a wire or knitting-needle, and no instrument could be passed into the bladder. An abscess formed in the perinæum, which was opened by Mr. Phillips. The patient was then transferred to the care of Mr. B. Holt, who commenced treating him with his stricture-dilator, Feb. 5, 1852.

Nothing larger than No. I could be passed at first. This, however, was got through every other day, but, owing to the appearance of some irritability and hæmorrhage, nearly a week's intermission in the use of instruments took place. The fourth time this dilatation was tried, the canal was found so much widened, the dilatation could be carried so far that a No. 6 was admitted. After this period, dilatation was steadily continued till towards the close of the month, when this report was drawn up. At this time, No. 9 could be passed with ease.

The man seemed very much pleased with the results of this treatment. The pain had been considerable at times, at others slighter; the abscess soon healed up, and the patient's health improved, especially as he could retain his urine in the night instead of being obliged to rise a dozen times to void it. These are satisfactory results in so short a time, and in a case where nearly the whole line of the canal was strictured.—*London Med. Timns.*

CHEMISTRY.

The State in which Oxygen exists in the Blood.—Liebig, in the *Chemisch-Pharmaceutisches Central-Blatt*, Dec. 1851, has published a memoir on this subject, in which he combats the opinion that oxygen is not chemically combined with, but is simply absorbed by the blood; a belief grounded on the fact of the expulsion of the absorbed oxygen by a current of carbonic acid passing through the liquid. The author denies the correctness of this inference on the following grounds: 1000 volumes of water, according to Gay-Lussac, when saturated with atmospheric air, absorb 9·25 volumes of oxygen, and 18·5 volumes of nitro-

gen; whilst, according to Magnus, 1000 volumes of blood absorb from 100 to 130 volumes of oxygen, and 17 to 33 volumes of nitrogen; therefore blood absorbs from eleven to fourteen times more oxygen than water does, which absorption must necessarily depend upon the existence in the blood of some particular substance, possessing a far stronger affinity for oxygen than water possesses. It does not follow because the the affinity of this principle in the blood for oxygen is but weak, that no chemical combination exists between them. One per cent. of phosphate of soda in water, doubles the absorbent power of the water for carbonic acid gas; sulphate of iron in solution dissolves forty times more than a similar bulk of pure water can; and both these solutions again part with their absorbed gases, when placed *in vacuo*, or by agitation of the former with air, the latter with carbonic acid; so that the conclusion is obvious, that saline solutions possess (*may possess*) greater absorbent powers than pure water—a conclusion which applies to the blood.

The absorbent power of a liquid for gases, depends either upon the pressure maintained, or the affinity of that liquid, or of one or more of the constituents of that liquid, for a given gas. Were the oxygen of the blood simply absorbed, it must take up from the air, which itself contains but one-fifth of oxygen, twelve per cent. of this gas; double this amount under twice the pressure; treble under thrice this amount of pressure; and five times the quantity if shaken with pure oxygen. It has not yet been proved that the absorbent power of blood varies with the amount of atmospheric pressure. Regnault and Reiset have demonstrated that animal life can be maintained in an atmosphere abounding with oxygen; and it is well known that in the neighborhood of Lake Titicaca, at an elevation of 12,600 feet above the sea level, 15,000 persons, and that at Potosi, an elevation of 12,600 feet, 30,000 persons live and breathe every whit as well as the inhabitants of the coast; facts which militate against the supposition that oxygen is merely *absorbed* by the blood; for as absorption is dependent upon pressure, the amount absorbed at different elevations would be dissimilar, which must necessarily exert considerable influence on the vital action in the mountaineers, or on the dwellers on the sea-shore—a conclusion utterly opposed to observation.—*London Journal of Medicine.*

Antidote for Cupreous Salts.—M. Roncha, of Strasburg, states that calcined magnesia completely checks the symptoms occasioned by poisoning with sulphate of copper, if taken quickly after the copper salt; but it requires a large quantity of magnesia, eight times the weight of the sulphate of copper, to neutralise the effects of this salt. It is stated as probable, that magnesia will prove one of the best antidotes to cupreous salts, a statement in which at present we should be sorry to express our acquiescence.—*Ibid.*

Test for the presence of Mercury.—If a strong solution of iodide of potassium be added to a minute portion of any of the salts of mercury, placed on a sheet of bright copper, a white metallic silvery stain will be

developed, which cannot be mistaken, as no other metal presenting a similar appearance is deposited by the same means. In this way corrosive sublimate may be detected, in a solution unaffected by potash or iodide of potassium. In a mixture of calomel and sugar, in the proportion of one of the former to two hundred of the latter, a distinct metallic stain will be detected with one grain of this mixture, containing but 1-200 of its weight of calomel, and the binoxide of mercury can be thus detected, if mixed with 400 times its weight of sugar. This valuable test we owe to Mr. Arthur Morgan.—*Ibid. from Annals of Pharmacy.*

On the Examination of Ointments containing Oxide of Mercury. By M. ROBIERRE.—Having had occasion to examine some citrine ointments employed by a woman who was accused of illegally practising medicine, I experienced some difficulty in ascertaining the chemical characters of the metallic substance present, in consequence of the small proportion of it contained in the ointment, and also in consequence of the ointment being very old. The following method of operating proved so successful, that I am induced to record it for the benefit of those engaged in such investigations. It serves to isolate mercury in a few minutes from its combination with oxygen and fatty acids. However small the quantity of mercury present may be, the effect is, nevertheless, distinct.

The ointment to be examined is melted by the application of a gentle heat, and a small quantity of the essence of citron is then added to it. Under the well-known reducing influence of this hydro-carbon, the ointment acquires a grey color, which effect is to be promoted by agitation. After about five minutes, the ointment being still kept melted, three times its volume of ether is to be added, the whole mixed together, and then allowed to stand. The supernatant liquid is then to be decanted, and the residue washed several times with ether. The mercury left at the bottom of the vessel may now be dissolved in nitric acid, and tested with the usual reagents.—*Journal de Chimie Médicale.*

On the Hydrated Peroxide of Iron and Magnesia as Antidotes in Poisoning with Arsenic. By J. HAIDLEN.—The author has satisfied himself by experiments, that to completely precipitate 1 part of arsenious acid within five minutes by hydrated oxide of iron, 22 parts of the latter are required. Some which had been kept for a year had lost more than two-thirds of its power; in another case it had lost one-third of it. As it is well known, on account of this property of the hydrated oxide of iron, Fuchs has recommended the mixture of a solution of persulphate of iron, which yields from 17 to 18 per cent. of the hydrated peroxide dried at 212° F., with excess of caustic magnesia. In the Wurtemberg Pharmacopœia the mixture of perchloride of iron with excess of magnesia is prescribed. The author finds that both mixtures precipitate the arsenious acid equally as well as the pure hydrated peroxide of iron, so that there is no chemical objection to their use.—*Chem. Gaz.* May 1852, from *Jahrb. fur Prakt. Pharm.*

MATERIA MEDICA AND THERAPEUTICS.

On Guaraná. By D. RITCHIE, Esq.—Guaraná is a substance used in Brazil as a medicine. It is prepared from the seeds of the *paullinia sorbilis*, a sapindaceous plant. The masses of which it consists are cylindrical or spherical, hard, uneven, brown or black on the surface; fracture conchoidal, unequal, resinoid; color reddish brown, like chocolate. It is liable to be adulterated with cocoa or mandiocca flour. Chemical analysis shows the presence of tannin, caffeine and guaranine.

The medicinal properties have been examined by M. Theodore Martius, and more particularly by Dr. Gavrelle, who was physician to Don Pedro in Brazil. By the vulgar it is held to be stomachic, antifebrile and aphrodisiac; it is used in dysentery, diarrhœa, retention of urine and various other affections. It stimulates, and at the same time soothes the gastric plexus of nerves. It reduces the excited sensibility of the cæliac plexus, thereby diminishing febrile action, and strengthening the stomach and intestines, particularly restraining excessive mucous discharges, increasing the action of the heart and arteries, and promoting diaphoresis. It is therefore indicated in fever, or reduced vital power from cold or prolonged wet, grief, too great muscular exertion, depression of spirits, long watching, and also in colic, flatulence, anorexia, nervous hemicrania, or in a dry condition of the skin. It is contra-indicated in a plethoric or loaded condition of the abdominal viscera, and in determination of blood to the head. It is said to increase the venereal appetite, but to diminish the fecundating power. It is a valuable remedy in irritation of the urethra and bladder, succeeding venereal or attending organic disease.

Guaraná, in the state of powder, is given in doses of ʒj. three or four times daily, with water and sugar, or with syrup and mucilage, conjoined with an aromatic. A convenient form is the extract, made by treating guaraná with alcohol; it may be exhibited in the form of solution or pills.—*London Journal of Medicine.*

OBSTETRICS.

Removal of a Pessary after forty-one years' residence in the Pelvis.—In the *Ohio Medical and Surgical Journal* for May, Dr. F. T. Hurxthal reports the case of a German woman, aged 73 years, who, in 1811, after a confinement, had prolapsus uteri; and upon consulting a midwife (in Germany) she introduced a ring into the vagina, which she said was made of wood covered with beeswax. It had never been removed, nor had she ever felt the least inconvenience from it, until, on the second day of an attack of pneumonia, whilst coughing violently, she felt some pain in the pelvic region, which increasing, was followed by an exceedingly fetid vaginal discharge. The pessary was removed with difficulty, by division of it by scissors. It was $3\frac{3}{8}$ inches in its diameter, and $2\frac{7}{8}$ in its transverse diameter. On removal, it presented the appearance of a roughened stone, in some parts over an inch in thickness. The recovery was speedy and complete.